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THE ROMAN WALL:

A RECONSIDERATION OF ITS PROBLEMS.

THE Roman wall, as everyone knows, stretches from Wallsend on the Tyne to Bowness on the Solway, a distance of about 73½ miles. It is generally regarded as a double mural fortification, consisting of a stone-wall on the north and an earth-wall, or, rather, a series of earth-walls, on the south. By recent writers the stone wall has been called "the murus" and the earthwork "the vallum". This nomenclature I adopt, not in acknowledgment of its accuracy, but because it has become customary. The murus is provided with a ditch along its north side, and the earth-walls, which are three in number, have also a ditch between the first and second, counting from the north. The vallum, it should be mentioned, is shorter than the murus by about 3½ miles at each end. For a considerable distance, a road, running between the two walls, and usually called "the military way", may be distinctly traced. At each end of the murus is a walled camp or station, and between these are fifteen others, most of which are in contact with either the murus or the vallum, or with both. The line of the murus is broken not only by the camps, but by a series of smaller fortresses, called "mile-castles", situated at an average distance of about a Roman mile from each other. Between every mile-castle and the next there have been three or four turrets or watch-towers, but of nearly all these the traces have entirely disappeared.

Further general description of these structures is not needed for my present purpose. The reader who desires fuller information about them may consult the third edition of Dr. John Collingwood Bruce's work, *The Roman Wall* (London and Newcastle, 1867), or his more accessible *Handbook to the Roman Wall* (London and Newcastle, 1885).

It was generally held by antiquaries of the older school that Agricola built or planned the camps, and a military way connecting them; that Hadrian built the vallum; and that Severus was the builder of the murus. Long ago, however, the theory was put forward that the vallum and the murus are parts of one design, and were planned at one time and by one person; but the Rev. John Hodgson, the author of the *History of Northumberland*, was the first writer who avowed the belief that Hadrian was the builder of both walls. In the last published volume of his great work, near the end of a lengthy and most valuable account of the wall, he says:

"In the progress of the preceding investigations I have gradually and slowly come to the conviction that the whole barrier between the Tyne, at Segedunum, and the Solway, at Bowness, and consisting of the vallum and the murus, with all the castella and towers of the latter, and many of the stations on their line, were planned and executed by Hadrian." (*Hist. Northumberland*, part II, vol. iii, p. 309.)

The volume from which this extract is taken was published in 1840. Eleven years afterwards Dr. Bruce published the first edition of his *Roman Wall*, in which a chapter, occupying twenty-four pages, is devoted to the question, "Who built the Wall?" His verdict is that Hadrian was the builder of both murus and vallum, and he says:

"It is difficult to conceive how any person can traverse the line of the barrier without coming to the conclusion that all the works—vallum, wall and fosse, turrets, castles, stations, and outposts—are but so many parts of one great design, essential to each other, and unitedly contributing to the security of a dangerous frontier." (P. 387.)

Two years later appeared the "second and enlarged" edition of Dr. Bruce's book, in which the chapter on the builder of the wall is retained. But in his new preface the writer mentions, as one of the results of his later visits to the mural region, "a thorough conviction of the correctness of the view maintained in the former edition, that the lines of the barrier are the scheme of one great military engineer." Presently he adds:

"Fortunately for the investigation of truth upon these and other interesting archæological questions, the attention of his Grace the Duke of Northumberland has been turned to the antiquities of the ancient Northumbria. The beautiful and very accurate survey of the Watling Street, between the Scottish border and the Tees, with all the camps and stations upon it, which has just issued from the press, is the fruit of his liberality;

a thorough examination of the station of Bremenium is now going forward by his orders; and he has further given directions for making a survey of the whole line of the barrier, from sea to sea. . . . The survey of the wall by so accurate a draughtsman and so zealous an antiquary as Mr. MacLauchlan will be an invaluable contribution to the materials for compiling the early history of Britain. Many of our views and theories will be tested by the severity of mathematical investigation; the whole of the line will be laid under the inspection of the student in his closet, so that many who are now prevented by distance from examining the subject will be able to do so, and give to the world the benefit of their observations; new camps will probably be discovered; the exact bearing of one part of the mural region upon another will be demonstrated, and the spirit and design of the whole brought out in a way which they have never yet been." (Pp. x-xi.)

All this was evidently written in anticipation that MacLauchlan's survey would confirm and support what I would venture to call the Hadrianic theory. In due time that survey was completed. The results were "printed for private circulation" in 1858. No one can read the Memoir by which MacLauchlan's maps are accompanied without discovering that the writer had made himself thoroughly conversant with everything that had previously been written about the wall, and that he had examined the arguments of Mr. Hodgson and Dr. Bruce with all possible candour. That he was aware of every fact and circumstance by which they had attempted to maintain their position, his numerous foot-notes abundantly attest. What, then, were the conclusions at which he arrived? He writes in anything but a dogmatic style. After declaring that his conclusions "in a great measure coincide with those of Horsley", he proceeds to say:

"It seems probable that the stations were made by Agricola, and walled at some subsequent period; [and] that the north rampart of the vallum was made at the same time as the stations, as a line of road (*via militaris*) between them, forming in itself at once a way and a line of defence raised above the level of the country.

"It is probable that the vallum was made by Hadrian, at all events, before the wall.

"The wall and castles may have been made or designed at the same time, after the walling of the stations, commenced, perhaps, by Severus, and finished, or repaired, by his successors." (*Memoir written during a Survey of the Roman Wall*, p. 89.)

It is unnecessary, at this point, to offer any comment on these conclusions, further than to remark that they are opinions which resulted from the most careful and analytical examination of the

walls, with all their adjuncts, which has yet been made. The reasons MacLauchlan puts forward in support of his conclusions will be discussed in a later part of this article.

Nine years elapsed between the issue of MacLauchlan's *Survey* and the publication of the third, and hitherto the last, edition of *The Roman Wall*. Therein the Hadrianic theory is avowed as distinctly as before. In a paragraph which mentions the measures which Hadrian's experience and military genius showed him were "necessary for rendering permanent the Roman dominion in the upper isthmus of England", we are told that "the chief of these was the construction of that chain of fortresses—linked by roads and covered by a wall—which it is the object of this work to describe" (p. 14). So, at last, we have the avowal that the whole "mural barrier"—earth-walls, stone-wall, fosses, stations, mile-castles, turrets, and military roads—is the work of Hadrian.

THE STATIONS.

It is due to Dr. Bruce to say that he elsewhere acknowledges that it "is more than probable" that Agricola "built some of the stations—those especially which command the passes between the north and the south" (*R. W.*, 3rd ed., p. 375). I have sought in vain, however, in his pages for an explicit statement as to which of the stations he believes to have been built by Agricola and which by Hadrian, and have, consequently, only scattered allusions by which to be guided. But in fairness to him I must not be influenced, on this point, by anything I find in his first and second editions, in both of which he appears at least to ascribe all the stations to Agricola in one passage, and all to Hadrian in another.¹ In the third edition, however, he mentions three of the stations which "are quite detached from both lines of fortification [the murus and the vallum], being situated to the south of them"; and adds that "these probably have been originally constructed by Agricola to defend the defiles near which they stand" (p. 61). The stations alluded to are Chesterholm (Vindolana), Carvoran (Magna), and Walton House (? Petriana). Of Halton Chesters (Hunnum) he elsewhere says, "There can be no doubt that the station was

¹ "He [Hadrian] did not attempt to regain the conquests which Agricola had made in Scotland, but prudently sought to make the line of forts [between the Tyne and the Solway], which that general had constructed in his second campaign, the limit of his empire." (*Roman Wall*, 1st ed., p. 11; 2nd ed., p. 9.) Compare this with the passage quoted above (p. 82) from p. 387 of the first edition, which is repeated on p. 364 of the second.

planted in its present position to guard the Watling Street" (p. 133); and, as he acknowledges that this road "was probably formed by Agricola" (p. 139), we are justified in regarding this station also as, in his opinion, one of that general's forts. Then of Chesters (Cilurnum) he declares, "There can be little doubt that Agricola first reared the station" (p. 145). Thus, five stations out of seventeen are allowed to have been *probably* built by Agricola. The first of these is Hunnum, 20 miles from Wallsend. The second is Cilurnum, $5\frac{1}{2}$ miles from Hunnum. The third is Vindolana, over 9 miles from Cilurnum. The fourth is Magna, more than $6\frac{1}{2}$ miles from Vindolana. The last is Walton House, $10\frac{1}{4}$ miles from Magna, and $21\frac{1}{2}$ miles from Bowness. Thus we have five camps to defend a frontier 73 miles in length, with 20 miles at each end totally unprotected, and with two intervening gaps, one of 10 miles and the other of 9.

With these measurements in mind, let us turn to the pages of Tacitus. Speaking of Agricola's second campaign in Britain, he says:

"At the return of the summer (A.D. 79) he assembled his army. On their march he commended the regular and orderly and restrained the stragglers; he marked out the encampments, and explored in person the estuaries and forests. At the same time he perpetually harassed the enemy by sudden incursions; and after sufficiently alarming them, by an interval of forbearance he held to their view the allurements of peace. By this management many states which till that time had asserted their independence were now induced to lay aside their animosity, and to deliver hostages. *These districts were enclosed with castles and forts, disposed with so much attention and judgment, that no part of Britain, hitherto new to the Roman arms, escaped unmolested.* (Quibus rebus multæ civitates, quæ in illum diem ex æquo egerant, datis obsidibus, iram posuere, et præsidiis castellisque circumdatæ, tanta ratione curaque, ut nulla ante Britanniae nova pars inaccessita transierit.)"

The question arises: Where were the forts and castles with which the subdued districts were enclosed? It is generally agreed that Agricola's second campaign was directed against the Brigantes, whose northern boundary was the isthmus between the Tyne and the Solway. Agricola, having conquered this tribe, *enclosed* their territory with castles and forts. Does this mean that he planted camps or stations at intervals throughout the whole of their frontiers? What would be the use of placing camps on the south boundary of their territory? The Cornavii and the Coritani had been previously subjected to Roman power. Then, what object

could Agricola have in fixing camps along the sea-shores of the Brigantes? Invasion by sea was not what he apprehended. His object, clearly, was to protect already conquered territory against the incursion of the inhabitants of districts yet unsubdued. This, I venture to think, is the conclusion at which any unbiassed reader of Tacitus would arrive. It is certainly the conclusion at which the author of *The Roman Wall* had arrived when he declared that Hadrian prudently sought to make the line of forts constructed by Agricola between the Tyne and the Solway the limit of his empire—an opinion which appears to have been abandoned in favour of an all-absorbing claim for Hadrian. At a later period, in a chapter contributed to Mr. John Hodgson Hinde's introductory volume of the *History of Northumberland*, Dr. Bruce says that the passage just quoted from Tacitus

"gives no authority for supposing that the strongholds erected by Agricola in his second campaign formed a *chain* across the isthmus of the north of England. It simply states that Agricola *dispersed* (!) his forts in such a way as best to overawe the people whom he had subdued during the course of the summer." (*Hist. Northumb.*, pt. 1, p. 28.)

Hereupon Dr. Bruce proceeds to speculate as to where Agricola's forts were planted :

"Two or three placed near the principal lines of march between the north and the south, would be as many as he would have time for in the immediate vicinity of the lower isthmus. Others would be planted to the south of it, probably on the great lines of road, and especially at those points (such as the passage of rivers) where the movements of a hostile force could be most easily impeded."

All this is quite consistent with the notion that Agricola *dispersed* his forts throughout a certain district, but is quite incompatible with the declaration of Tacitus that that great general enclosed that district with forts. The verb used by Tacitus is *circumdo*, and the liberty taken is very great when this word is made equivalent to the English verb *to disperse*. To the assertion, therefore, that the passage in Tacitus "*simply states* that Agricola *dispersed* his forts", we must unhesitatingly object. Tacitus says that Agricola *enclosed*, or *surrounded*, certain districts with castles and forts, and, as a prudent general, he would enclose them in this way where they needed to be enclosed, that is, along the frontier beyond which were the yet unconquered tribes. This was the northern frontier of the territory of the Brigantes—the frontier commanded by the stations between the Tyne and the Solway.

In a passage just quoted, the author of *The Roman Wall* supposes

that two or three forts between the Tyne and the Solway would be as many as Agricola in his second campaign would have time to plant; though this number, by acknowledgment of several "probabilities", he elsewhere increases to five. But of what use would five forts be to guard a frontier 70 to 80 miles in length? Some of these forts, it may be granted, guarded the great military roads of the Romans. But what were these roads to the native tribes of Britain? Did the Caledonians wait till the Romans had constructed military roads before they invaded the conquered districts? The roads were formed to enable the Romans to carry out their own plans of conquest, and to maintain what they had already gained. But had the northern tribes confined their movements to the military roads of their enemies, there could have been no necessity for the erection of either the vallum or the murus. If, then, Agricola fortified this frontier at all—and that he did so, the passage from Tacitus affords conclusive evidence—it must have been by an efficient line of camps, such a line as we cannot admit the five which Dr. Bruce thinks were probably planted by Agricola to have constituted.

I do not contend that absolutely every station along the line of the wall is the work of Agricola. Pons Ælii (Newcastle), for instance, is apparently ascribed to Hadrian by its very name—a name which implies a relationship between bridge and camp which must not be overlooked. And if we omit this station from the list, we find that the distance between Wallsend and Benwell accords much better with the average distance of stations along the wall. Taking the stations from Wallsend to Birdoswald (Amboglanna), which is as far westward as we can certainly identify the names in the *Notitia* list, and omitting Pons Ælii, Vindolana, and Magna—the first, because it appears to be an addition to the series, and the two latter because they lie south of the line of the wall—we get an average distance from station to station of 5 miles $3\frac{1}{2}$ furlongs. It is a singular fact that in the Ravenna list both Pons Ælii and Vindolana are omitted, though all other stations from Segedunum to Æsica occur in their proper order. Unfortunately, the Ravenna list beyond Æsica is confused and incomprehensible, but it is certain that it makes no mention of Magna. These omissions from this list are of very little, if any, value as evidence, but they are at all events remarkable, and worthy of note. The greatest distance between any two stations is that from Walton House to Stanwix, which is 7 miles $7\frac{1}{2}$ furlongs; and if, besides Pons Ælii, Vindolana, and Magna, we omit Drumburgh, as I think we ought,

the shortest distance is then between Cilurnum and Procolitia, which is only about $3\frac{1}{2}$ miles. But taking their average distances, I think there is clear evidence of an intention on the part of their founder to fortify the north frontier of the territory of the Brigantes with stations planted, as nearly as the contour of the country would permit, at distances of about six Roman miles from each other.

In reply to all this it may be said that the contemporary date of the murus and many of the stations is proved by the parallelism of the murus itself, as it approaches one or both sides of a station, with that station's north and south walls. In other words, the fact that in a few cases the murus comes up to the east or west wall of a station, at a right angle to that wall, proves unity of design. The only stations about which this can be said are Condercum, Vindobala, Hunnum, Cilurnum, and Procolitia. Two of these, however (Hunnum and Cilurnum), Dr. Bruce regards as the work of Agricola, and therefore, so far as he is concerned, the argument with which I am now dealing is impotent. But it is sufficient, in reply, to say that the murus is only parallel with the north and south walls of a station when it is also parallel with the vallum; and this seems to afford ground for believing, with Mr. MacLauchlan, that one member of the vallum was contemporary with the stations, and was constructed to serve as a military way between them. But however this may be, the fact that when the murus is not parallel with the vallum it is never parallel with the north and south walls of a station, disposes of any argument for unity of design which may be based on such parallelism. At neither Wallsend nor Bowness—stations which the vallum never reached—does the murus come up to the station at a right angle to the wall to which it joins.

Both Mr. Hodgson and Dr. Bruce are willing to allow that some of the stations, which they believe were planned by Hadrian, were actually built before the murus, although, they contend, the interval was a short one. Hodgson, for instance, speaking of Amboglanna, says that the murus, "though it forms a straight line on its north side with the rectilinear part of the north wall of the station, is not tied into that wall, *but built of much larger courses of stones, and much more rudely than it*; and thus evidently proves that it was of later construction than the station itself" (*Hist. Northumb.*, ii, iii, p. 207). He also mentions that "the north-east corner of Æsica is also [like that of Amboglanna] rounded off, and apparently not tied, like the side walls of the castella [the mile-castles], at right

angles, into the murus ; and thus, in both cases, the stations seem to have been built prior to the murus" (p. 307). This acknowledgment of the earlier date of the stations is, however, only intended to mean that Hadrian's builders constructed the stations before they began to erect the murus ; for the same writer also declares, "Borcovicus and Æsica, I would say, are manifestly coeval with the murus" (p. 278). On this point Dr. Bruce is at one with Mr. Hodgson. Of Borcovicus he says : "No one can doubt that the station was rendered complete before the wall was annexed to it ; and yet no one who examines the whole subject will fail to see that but for the wall the station of Borcovicus would never have existed" (*Roman Wall*, 3rd ed., p. 181). I take the meaning of this sentence to be that the station was built with the intention of "annexing" the murus as soon afterwards as possible. Of Æsica, he says : "The station was probably quite independent of the wall. . . . At the same time it cannot for a moment be supposed that but for the wall this station would have had an existence" (p. 233). And of Amboglanna he declares, "The station has been built entirely independent of the great wall" (p. 256). How the station could be built "entirely independent" of the murus, it would be hard to understand, were not the writer's meaning elucidated in other passages. One thing, however, is quite clear. Both Mr. Hodgson and Dr. Bruce admit that two or three of the stations, which both believe to be parts of Hadrian's design, were built before the murus, and we are left to gather evidence as to the length of time which intervened.

In all cases which can be ascertained all the four corners of the stations are rounded. Now, at Procolitia,¹ Borcovicus, Æsica, and Amboglanna the murus comes up to the north-east and north-west angles of the station. At Borcovicus both north angles, and at Amboglanna one of them, are still seen to be rounded, and there is evidence that they were rounded at Æsica. This, however, is certainly an arrangement which would never have been adopted had

¹ Of Procolitia Mr. MacLauchlan says : "Though the general outline of the station is easily made out, the wall of it is not visible, hence the true bearing and position is somewhat uncertain ; but the bearing of the north wall of the station is about one degree more towards the north than that of the great wall, and about three degrees more than that of the vallum." And in a footnote he adds : "This want of conformity in structure would lead to a supposition that the station was built before either the vallum or the wall ; and it is not impossible that the two north angles were rounded like the two south ones, and destroyed in the construction of the wall, which probably formed nearly a straight line with the north front" (*Memoirs, etc.*, p. 34).

it been the intention of the builder that these corners should be continued into an adjoining wall. The way in which the murus abuts upon the north corners of the stations just mentioned¹ has forced from Mr. Hodgson and Dr. Bruce the acknowledgment that these stations at least are earlier in date than the murus; although, they contend, they are coeval in design. But anyone not already wedded to a theory would see that this rounding of the north corners of these stations contemplated their permanent exposure, and is thus evidence that the murus did not enter into the plan of their designer. In this respect we may compare the stations with the mile-castles. The latter, it is agreed by everyone, are part of the design of the murus, and are coeval with it. *In all cases which can be ascertained the south corners of the mile-castles are rounded, but their north corners, which are in contact with the murus, are rectangular.* The conclusion is irresistible. Had the north corners of Borcovicus, Æsica, and Amboglanna been intended by their builder or designer to have the murus annexed to them, they would have been rectangular also.

But that Agricola planned the principal stations from the Tyne to the Solway we have evidence from their analogy to those which he constructed in Scotland. We have seen that his camps on the north frontier of the territory of the Brigantes were the work of his second campaign, in A.D. 79. Turning again to the pages of Tacitus, we read:—

“The military expeditions of the third year [A.D. 80] discovered new nations to the Romans, and their ravages extended as far as the estuary of the Tay. The enemies were thereby struck with such terror that they did not venture to molest the army, though harassed by violent tempests; so that they had sufficient opportunity for the erection of fortresses. Persons of experience remarked that no general had ever shown greater skill in the choice of advantageous situations than Agricola, for not one of his fortified posts was either taken by storm or surrendered by capitulation. The garrisons made frequent sallies, for they were secured against a blockade by a year's provision in their stores. Thus the winter passed without alarm, and each garrison proved sufficient for its own defence; while the enemy, who were generally accustomed to repair the losses of the summer by the successes of the winter, now equally unfortunate in both seasons, were baffled and driven to despair. . . .

“The fourth summer [A.D. 81] was spent in securing the country which had been overrun; and if the valour of the army and the glory of the Roman name had permitted it, our conquests would have found a limit

¹ See Ordnance Maps, 25 in. scale, *Northumberland*, lxxxiii, 12, and *Cumberland*, xii, 12.

within Britain itself. For the tides of the opposite seas, flowing very far up the estuaries of Clota and Bodotria, almost intersect the country, leaving only a narrow neck of land, which was then defended by a chain of forts. Thus all the territory on this side was held in subjection, and the remaining enemies were moved, as it were, into another island."

The estuaries of Clota and Bodotria are the Firths of Clyde and Forth. The "chain of forts" constructed by Agricola between these estuaries is universally allowed to be the series of camps, afterwards connected to each other by an earth-wall, under the direction of Antoninus Pius and his legate, Lollius Urbicus. This wall is analogous to the English vallum, but in no sense to the murus. But both Mr. Hodgson and Dr. Bruce admit that the stations and wall of Antonine's barrier are not parts of one design, and are not coeval in date. Yet the Scottish wall, as anyone consulting the maps in Stuart's *Caledonia Romana* may see, pursues a far more direct course than the English wall. This, however, is not all. A careful examination of the line pursued by the Antonine wall shows fewer deflections for the purpose of coming up to its stations than will be noticed in the course of the English wall. This is all the more remarkable when we remember that the Scottish camps are, on an average, a little under two miles from each other. But an examination of the line of the English wall will, I think, dispose us to agree with Horsley, who, ascribing the stations to Agricola, the vallum to Hadrian, and the murus to Severus, says: "We find the course of the wall directed, as much as it well could be, from station to station, and making some turns with no other view but to come up to and fetch in a station." (*Britannia Romana*, p. 98.)

VALLUM AND MURUS: EVIDENCE OF THE STRUCTURES.

We now come to discuss the relative dates of the vallum and the murus. Before examining the evidence of inscriptions and ancient historians on this point we must consider that which is afforded by the structures themselves. On this subject Dr. Bruce says:

"A careful examination of the country over which the wall runs almost necessarily leads to the conclusion that whilst the wall [*i.e.*, the murus] undertook the harder duty of warding off the openly hostile tribes of Caledonia, the vallum was intended as a protection against sudden surprise from the south." (*Roman Wall*, 3rd ed., p. 59.)

He also says :

"One circumstance, admitted by all writers on the wall (?), argues very strongly in favour of the theory that both murus and vallum are but parts of one great engineering scheme: it is that the wall is almost uniformly taken over ground which gives the greatest advantage in repelling an attack from the north, and the vallum occupies positions whence a southern foe could best be resisted." (P. 380.)

Dr. Bruce has other arguments, drawn from the works themselves, in support of his theory, and these will be presently noticed. But, in reply to the passages just quoted, it may be said, and said truly, that throughout a very great part of its course the vallum is equally well adapted, so far as its mere position is concerned, to resist attack either from the north or from the south. Then, also, there are many places in which the vallum is commanded by higher grounds on the south. Of these it will be sufficient to give two or three instances. A little more than half a mile west of the bend in the wall at Heddon-on-the-Wall, the vallum is overlooked by an eminence on the south, about 100 feet higher than itself, and at a distance of less than two furlongs (*Northumb.*, lxxxvii).¹ Opposite the mile-castle at Cawfields Crag, near Æsica, at a distance of about five furlongs south of the vallum, the land is more than 170 feet higher than the vallum itself (*Northumb.*, lxxxiii, xcii). Near the station of Æsica, the land south of the vallum rises rapidly, attaining, at a point about two furlongs west, an elevation of nearly 130 feet above it, and this at a distance of not more than a third of a mile (*Northumb.*, lxxxii, xci). From High Shield, a mile and a half west of Borcovicus, to "Twice Brewed", a distance of three-quarters of a mile, the vallum commands the north, but not the south, and this is perhaps a more decisive instance than those in which it is commanded by much greater heights, for here the land slopes gently down to the south rampart, although the vallum itself, by being carried a little further to the south, would have occupied the highest ground (*Northumb.*, lxxxiii). If, at this point, the vallum were defended from the north, the defenders would have their enemies above them. About a quarter of a mile east of "Twice Brewed", where all the members of the vallum, except the middle rampart, are well defined, the summit of the south rampart, in consequence of the contour of the ground, is much higher than that of the north one (*ibid.*). And this is also the case three-quarters of a mile further west, where

¹ This and similar references are to the six-inch scale Ordnance Maps.

the vallum leaves the road just past the inn at Low Winshield, with this additional fact, that the field on the south side of the south rampart gently rises from it (*ibid.*). In all these cases the vallum commands the north, and could be defended with ease and advantage from its south side, but could not be defended at all from the north. And it must be borne in mind that these are all instances in which, had the object of the constructors of the vallum been to make it defensible from the north, they might have done so by deflecting it one or two furlongs from its present course, and over ground which it would have traversed with equal ease.

But if the murus and the vallum were parts of one design, it must surely have been necessary that the troops garrisoning the former should be able to see if their services were needed along the line of the latter. Dr. Bruce recognises this, and assures us that "they [murus and vallum] are never so far removed that one may not be seen from the other" (*Hist. Northumb.*, i, p. 45). Nevertheless, there is one place, about a mile and a half west of Borcovicus, where, for a considerable distance, in consequence of an intervening ridge of basaltic rock, the one barrier *cannot* be seen from the other. This is opposite the mile-castle at Hot Bank (*Northumb.*, lxxxiii).

And this leads me to say that, throughout their whole course, the murus and the vallum wander furthest from each other where, if they were parts of one design, it would be necessary that they should keep nearest together, and are actually nearest where, with the greatest safety, they might have been furthest apart. West of Borcovicus, for instance, where they are as far apart as at any point throughout their course, the greater part of the intervening space is rough, rocky moorland of steep gradients, over which it is not possible to walk either rapidly or easily. Most of the remaining part is swampy morass (*Northumb.*, lxxxiii). Surely no engineer, forming two lines of fortification intended to be generally parallel to each other, would carry them at a distance of nearly half a mile apart over a district of this character, and yet, when the ground is level and easily traversed, bring them within a distance of 60 yards from each other, and sometimes even less. If the Hadrianic theory were sound, it should show us that the builder of the two walls provided for easy access from one barrier to the other, and for this purpose brought them nearest where the ground was most difficult of passage, and carried them further away where it was most easily crossed. But, in fact, the very opposite of this is the case; and this alone, without any additional evidence, is sufficient to disprove the theory of unity of design.

But before we leave the discussion of this part of the question, it may be desirable to examine another statement by Dr. Bruce, bearing on the same points. After repeating the assertion that "whilst the ground along which the wall runs is generally the strongest which could be chosen for a protection against the north, that over which the vallum goes is better adapted for repelling invasion from the south", he proceeds to say :

"In many instances the vallum takes the southern slope of a hill when it might as easily have embraced its northern margin. . . . Throughout the entire distance extending from Sewingshields to Carvoran [*Northumb.*, lxxxiv, lxxxiii, lxxxi, xci], the vallum runs along the southern slope of the basaltic hills on the northern escarpment on which the murus is situated, and is commanded throughout by the rising ground to the north." (*Hist. Northumb.*, i, p. 46.)

If I wished to point to one part of the mural district as affording stronger evidence than any other against this theory of unity of design, it is the portion between Sewingshields and Carvoran. Here, *in many instances, the vallum takes the northern slope of a hill when it might as easily have embraced its southern margin.* And instead of running "throughout the entire district" on the *southern* slope of the basaltic hills, it very frequently, as we have previously seen, runs along the *northern* slope of the opposite hills, and occasionally along the very lowest part of the valley. Dr. Bruce admits that the murus is a better defence against the northern foe, than is the vallum against a southern one, and explains or excuses this by saying :

"As the Caledonian foe was more to be dreaded than the subject Brigantes, the wall must of necessity be made to seize every advantageous point, no matter how frequently it changes its direction ; while the vallum, as having a less arduous duty to fulfil, may be allowed to proceed for a considerable distance in the same straight line, irrespective of occasional disadvantages, provided its course on the whole be suited to its purpose." (*Hist. Northumb.*, i, p. 45.)

The "occasional disadvantages" to which Dr. Bruce refers are the parts of the vallum where it is overlooked by higher grounds on the south. But, surely, anyone will admit that had the vallum been designed, for the purpose of resisting a southern enemy, by the same great engineer who built the murus, instead of being carried, as it is frequently between Sewingshields and Carvoran, along the very foot of the valley, or on the northern slope of the opposite hills, it would have been drawn with unvarying uniformity along the southern declivity of the range on which the murus itself

is built, and as near as possible throughout to that more important and efficient structure.

There are, however, several other arguments on which the Hadrianic theory depends for evidence of "unity of design" in the murus and vallum, the principal of which it is necessary to examine. One of these is founded on the relation of the vallum to the stations. Dr. Bruce states this argument in the following words :

" In a very few instances the stations are to the south both of wall and vallum, but in the majority of cases the vallum [supposing it to have been built before the murus] leaves the stations to a large extent or altogether unprotected. This is a state of things which no engineer of ancient or modern times would tolerate. View the whole as one work, and the difficulty vanishes. The murus comes up to the north rampart of the stations, or the northern pier of their lateral gateways ; the vallum falls in with the southern rampart, or the southern pier of the gateways. Thus the vallum and the wall bind the whole fortification together, and enable it satisfactorily to resist aggression from whatever quarter it may come." (*Roman Wall*, 3rd ed., p. 381.)

This argument is based on a generalization which requires correction. Most persons, reading the paragraph just quoted, would come to the conclusion that, except the "very few instances" in which "the stations are to the south both of wall and vallum", the murus "comes up to the north rampart of the stations or the northern pier of their lateral gateways", whilst "the vallum falls in with the southern rampart or southern pier of the gateways." Seventeen stations are usually regarded as belonging to the mural barrier. Three of these are south of both murus and vallum. Of the remaining fourteen, two afford no evidence as to how they were approached by the murus. Twelve stations are left, and of these the murus comes up to the north wall of seven and to the lateral gateways of five. But Dr. Bruce's statement is more incomplete in reference to the relation between the vallum and the stations. Again omitting the three stations which lie south of both barriers, we must remember that the vallum never reached the two stations at the extremities of the wall. This reduces the number to twelve, and of these we have no evidence in four cases as to how they were approached by the vallum, if, indeed, some of them were approached by it at all. In two instances of the remaining eight the vallum does not approach the station, and of the small residue of six the vallum comes up to the south rampart of five, and to the lateral gateways of *one*. Certainly six stations out of seventeen is a small proportion from which to draw a generalization. But, as it seems to me,

all this difference in the way in which the stations are approached or not approached by the murus and vallum, is evidence against the theory of unity and design. Roman works, in England and elsewhere, usually show marked evidence of what I might call rigid uniformity of plan; and had the stations, vallum, and murus been constructed at one time, and originally formed parts of one design, there can scarcely be a doubt that many of the dissimilarities which are so observable in the relation between the three great lines of fortification would not have existed.

The argument for unity of design is sometimes singularly inconsistent. When, in the paragraph just quoted, the writer contends for this unity from the way which the stations are approached by the murus and the vallum, he apparently forgets that he has ascribed two of the stations—Hunnum and Cilurnum—at which the murus comes up to the lateral gateways and the vallum to the south rampart, to Agricola. Elsewhere he says: "The manner in which the two walls combine in giving strength to a station is very well shown in Warburton's plan of the works in the vicinity of Cilurnum"; which plan he re-engraves, and quotes with especial approval Warburton's declaration that station, murus, and vallum "must have been one entire united defence or fortification" (*Roman Wall*, 1st ed., p. 390; 2nd ed., p. 366). But the station itself, Dr. Bruce grants, was built by Agricola. Granting, for a moment, that Hadrian built both vallum and murus, it is evident that he or his engineers found no difficulty in adapting their works to the existing station, so as to form altogether "one entire united defence or fortification". But is there any greater difficulty in believing that Severus adapted his murus, at Cilurnum and other similar places, to an already existing station and vallum, than in assuming that Hadrian adapted both murus and vallum to an existing station? Certainly not. Thus the argument for unity of design, based upon the manner in which some of the stations are approached by the murus and the vallum, is broken down by the acknowledgment that two of the stations at which this evidence of unity of design is especially evident were built before either of the walls was thought of.

But, it is fair to say, the principal point in the argument we are considering yet remains to be answered. It is contended that, if the vallum was built before the murus, the stations derived no advantage from its construction, but were left "to a large extent, or altogether, unprotected". The purpose for which the vallum was formed yet remains to be discussed, and, at present, it may be suffi-

cient to inquire, What advantage would the stations have derived from the vallum had its course been on their north side? Three of the stations have the vallum on the north, and reasons might easily be given for this arrangement in these instances; but what line must the vallum have taken to leave the stations of Borcovicus and Æsica south of itself? Along the Antonine wall almost all the stations are on the south side. But there, the ramparts of the camps, like the wall itself, are of earth. But the ramparts of the English stations, along the line of the wall, are of masonry, and therefore would have been in no way strengthened by being enclosed by the vallum. This fact alone affords a sufficient reason for the vallum being generally constructed south of the stations.

But here it is necessary to notice a curious fact to which Dr. Bruce draws our attention. Speaking of the vallum, he says:

"No apparent paths of egress have been made through these southern lines of fortification. The only mode of communication with the country to the south, originally contemplated, seems to have been by the gateways of the stations." (*Roman Wall*, 3rd ed., p. 58.)

To the gateways of the stations we must, of course, add the three great Roman roads which crossed the wall. As we have already seen, there is only evidence in five cases that the vallum coincided with the south rampart of the station. How the vallum approached Pons Ælii and Drumburgh, *its* two terminal stations, we do not know; but including the two intermediate stations, as to its relations to which we have no evidence, we get a total of ten means of ingress and egress through the vallum, in the whole 67 miles of its course. With the Roman troops on its north side it must have been a singularly inefficient defence against the revolting Brigantes. Except by five, six, or seven gates, and by three roads in 67 miles, the Romans had no means of getting at their enemies, unless they scrambled over three dykes and through a ditch. How different were the facilities for getting at the Caledonians! Every mile-castle and station had a northern gateway. But the vallum would be equally as advantageous to the revolting Brigantes as to the Romans, if not more so. When we come to consider the structure of the vallum we shall see that, quite irrespective of the stations, it is better calculated to resist a northern foe than a southern one. The theory that it was formed as a defence against rebellious Brigantes may be dismissed with the remark that, when that tribe had been subjected to the Roman arms, provision was made against its subsequent revolt by planting stations and camps throughout its

whole territory. Let anyone look at the map of Roman Britain and see how the territory of the Brigantes, from the Humber and the Mersey to the Tyne and the Solway, is dotted with cities and innumerable camps, and intersected by military roads, and then say whether, had the Brigantes rebelled, the power to quell them must not have been the military forces stationed in the cities and camps which had been planted in their own territory. And had this power failed—had the native tribes of Yorkshire and Lancashire once mastered the Roman troops that dwelt amongst them, and then rushed forward to this earthen rampart, not all the cohorts and *alæ* stationed along the wall could have resisted them. The vallum itself would have aided them. Planting themselves on the smallest rampart, which fringes the south side of the ditch, they would have cut off the Roman soldiers as they attempted to scramble through it, whilst from many a favouring height, and even from the south rampart of the vallum, they would hurl their spears and let fly their arrows upon their foes.¹

If, however, the vallum was formed for defence against a southern foe, it may be asked, What was it that was to be defended? Whilst the great mission of the Roman troops that manned the wall was to defend the territory of the Brigantes, and

¹ In the thirteenth volume of *Archæologia Eliana*, Chancellor Ferguson, of Carlisle, puts forward a new theory of the purpose of the vallum, which is chiefly valuable as a tacit confession of the weakness of the previous theories of those who, like himself, Mr. Hodgson, and Dr. Bruce, wish to make wall and vallum "the work of one mind, carried out simultaneously". Mr. Ferguson holds that the vallum was constructed, not as a defence against revolting Brigantes, but against the attacks of *guerillas*, *banditti*, and *dacoits*, who, he believes, infested vast and almost impenetrable forests of underwood, which he describes by the scarcely classical term *scrub*. It is, however, a great tax upon our credulity to expect us to believe that, during the Roman occupation of Britain, the immediate rear of the Roman wall, from one side of the island to the other, was an unbroken region of "scrub". But if this even were the case, it appears to me, and will, I daresay, appear to most people, that to clear an open space of ground, of moderate breadth, would have been a sufficient defence. Mr. Ferguson pictures "some veteran centurion *hirsutus et hircosus*", guarding the palisaded vallum, who, "since there were no gates in the palisades" for the *guerillas*, the *banditti*, and the *dacoits* to come through, "could be trusted to see they did not come over." The picture is almost grotesque. One sees the palisaded rampart and the "veteran centurion" behind it, with the vast interminable "scrub" on the other side, and the *guerillas*, the *banditti*, and the *dacoits* lurking hither and thither amongst it, and ready at any moment to jump over into the clover of "the mural barrier", should they fail to scent the *hircosus* centurion; and then, one wonders what the two unpalisaded ramparts and the ditch were for, and whether they were on the side of the "veteran centurion", or on that of the *guerillas*, the *banditti*, and the *dacoits*.

that of even more southern tribes, against the invasions of the Caledonians, were they, on occasion, to reverse their calling, and defend the Caledonians against the Brigantes? Or will it be said that their vocation was to defend themselves and the narrow strip of land included between the two walls?

But we have not exhausted the arguments by which it is attempted to be shown that the works themselves offer evidence of unity of design. One of these arguments Dr. Bruce states in the following words:

"If the murus does not maintain the same accurate parallelism with the vallum, which the several parts of the vallum do with one another, these two great works never cut in upon one another. The murus never crosses the vallum. If the wall had been constructed nearly a century after the vallum, and in consequence of the inefficiency of that rampart, would it not have pursued an independent course across the country, sometimes crossing the vallum in one direction, sometimes in another, occasionally diverging from it to a considerable distance?" (*Hist. Northumb.*, i, p. 45.)

That the murus occasionally diverges from the wall "to a considerable distance", no one who has seen its course from Sewing-shields to Carvoran will question; and the murus has done this so unscrupulously that, had still greater divergence offered any advantage, it is equally certain it would have left the vallum still further distant. But there can be no weight in the assumption that, had the murus and the vallum been independent works, they must necessarily have crossed and re-crossed. The existing arrangement presents no difficulty to those who believe that vallum and murus are works of different periods. The military engineer who planned the murus doubtless saw the inefficiency of the vallum, from its failing to seize the heights which were most advantageous for its purpose—heights in many cases practicable for a stone-wall, but not for an earthen rampart—and gave his orders that the new work should throughout be built to the north of the existing defence.

Hodgson puts forth an argument for unity of design in the murus and vallum which must not be passed over, especially as it is quoted approvingly by the author of *The Roman Wall* (1st ed., p. 389; 2nd ed., p. 365). The historian of Northumberland says:

"It [the vallum] and the murus *always* contract the width of the interval between them as they approach a river, apparently for no other purpose than a close protection of the military way and the defence of one bridge; for if they had passed the brooks and rivers on their line at any consider-

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able distance from each other, two bridges would have been necessary, and two sets of guards to defend them; and here it is not unimportant to remark that the murus always takes that brow of the ridge it traverses which is precipitous to the north, and never deserts its straightest or most defensible course to find a convenient situation for a bridge, while the vallum almost invariably bends inwards as it approaches a bridge, and diverges outwards as it leaves it." (*Hist. Northumb.*, ii, iii, p. 309.)

To this we may take MacLauchlan's reply. He says:

"The observation, that 'the vallum and murus always contract the width of the interval between them as they approach a river, apparently for no other purpose than a close protection of the military way and the defence of one bridge', in support of the idea that 'the works themselves furnish us with the best proof that the whole is one design, and the production of one period', will not hold good, for, as the works approach the River Tyne they slightly expand on each side [*Northumb.*, lxxxv]. In approaching the Tipalt they are nearly parallel on the east and present a slight contraction on the west, but leaving 100 yards between them [*Northumb.*, xci]; at the Poltross Burn they contract on the east and expand on the west [*ibid.*]; at the Irthing the vallum is obliterated, but, when last seen together, near Mumpshall on the east, expanding towards the stream; at the Cambeck it is found expanding on each side, if the line suggested for the vallum be admitted; and lastly, at the Eden they expand on the east and contract on the west, if it be granted that the vallum joined the castle at Carlisle [*Cumb.*, xxiii]." (*Memoir, etc.*, p. 90.)

"The evidence afforded by the works themselves", even in those points which are most relied upon as proof of "unity of design", is thus seen, when carefully examined, to break down completely; and it now only remains, in this part of our inquiry, to ascertain what other evidence pointing to difference of date of the vallum and the murus these works afford.

Mr. MacLauchlan mentions

"three places where the close approach of the wall to the vallum would seem to militate against their being of contemporaneous construction. The first is at Highseat, about three-quarters of a mile west of Rutchester [*Northumb.*, lxxxvii]; the second is about a mile west of Carraw [*Northumb.*, lxxxiv]; and the third about a mile east of Irthing [see MacLauchlan's map]. In these cases the wall approaches unusually near to the vallum—the mean distance between them being about sixty yards—where no natural obstacles intervened. This admits of the best explanation, perhaps, by supposing that the vallum occupied the ground previously." (*Memoir, etc.*, p. 90.)

But besides the instances alluded to by Mr. MacLauchlan, there are at least two others. The first of these is about a mile west of

the bend in the wall at Harlow Hill (*Northumb.*, lxxxvi), and the second is at Chapel Field, nearly two miles west of Walton House (*Cumb.*, xvii). In the first case the vallum continues in an almost perfectly straight line for nearly five miles, whilst the murus, after bending to the south at Harlow Hill, runs forward in a straight line until, near the middle of the distance just named, it is again compelled to change its direction by the presence of the vallum. The second case occurs at a point at which both murus and vallum make considerable angles, but where the two works approach much nearer each other than it is easy to believe they would have done had they been contemporary structures.

I must, however, again quote Mr. MacLauchlan. He says :

"There are also instances along the line where the wall appears to have been turned in its course for no other reason than to avoid running in upon the vallum, on the supposition that the latter had been made first; as may be seen about 400 yards east of Heddon-on-the-Wall, where the north rampart of the vallum is continued on to the height before the line is changed, similarly to the lines of the Watling Street; but the wall, approaching the same height, continues its course till obliged to change it, or to have run in upon the vallum in the hollow below [*Northumb.*, lxxxvii]. Again, at Newtown, about a mile west of Petriana, the wall approaches unusually near to the vallum, where the latter runs straight for a great distance, leading to an idea of independent construction" [*Cumb.*, xvii]. (*Memoir*, p. 90.)

A similar instance occurs about two miles north-east of Stanwix (*Cumb.*, xvi, xvii).

To all this Dr. Bruce has a very brief reply. Acknowledging that "observations coming from so high and unbiassed an authority" as Mr. MacLauchlan "will command universal respect", he proceeds to declare that "surely a few cases such as those named are not sufficient to overthrow the argument deducible from the design and execution of the works as a whole" (*Roman Wall*, 3rd ed., p. 382). We have already examined the "argument deducible from the design and execution of the works as a whole", and have seen that it is no set-off against the import of "a few cases such as those named". The argument for unity of design drawn "from the works as a whole" having in fact broken down, the "few cases named" remain unanswered evidence that the vallum and the murus are works of different periods.

I must now draw attention to a part of the mural barrier which affords even more decisive evidence against the theory of unity of design than any yet adduced. The Ordnance Map (*Cumberland*,

sheet xxiii) represents the murus and the vallum as running side by side in one place, for a distance of 400 yards, with no intervening space whatever between them. This is at what is known as Davidson's Banks, rather more than a mile west of the Castle of Carlisle. There is a reason for such close contact here which does not apply anywhere else throughout the whole line of the wall. The vallum, before the erection of the murus, ran along the south bank of the Eden, leaving a very limited space between itself and the river. When the murus came to be built north of the vallum, this could only be accomplished by leaving actually no available space between the two works. Mr. MacLauchlan does not show the site of the vallum at this point, but the site he ascribes to the murus is identical with that shown on the Ordnance Map. But he also shows the murus and the vallum coming into actual contact at the west end of Davidson's Banks. As the murus and the vallum come up to this place from the west, both MacLauchlan's map and that of the Ordnance Survey represent them as meeting at a very acute angle. Whether they continued in contact for 400 yards, or only for a single yard, does not materially affect the argument. I do not feel that I can call in question the twofold evidence of MacLauchlan's and the Ordnance maps, that the murus and the vallum, once at least in their course, came close together; and, if this one fact only be admitted, the theory that the one work was intended as a defence against a northern foe and the other against a southern rebellion, with all other theories of unity of design and contemporary date, falls to the ground. I grant that it may not be possible to gather on the spot, at the present time, any very satisfactory evidence of this contact of the murus and vallum near Carlisle. But it must be remembered that thirty-five years have elapsed since the completion of MacLauchlan's survey, and twenty-two years since the Ordnance survey of that district was made. Evidences which might be unmistakable twenty-two to thirty-five years ago, may be totally obliterated now. I know that, within a fourth of the shortest of these periods, considerable evidences of the vallum and of the ditch of the murus have been destroyed on the west side of Newcastle. It is, however, beyond all question that, at the part of the bank of the Eden under discussion, the vallum ran over ground which it never would have done had it been designed or constructed at the same time as the murus.

The very section of the vallum, however, quite apart from any question whether its north rampart was originally a military way, suggests that it was intended to resist a northern rather than a

southern foe. It consists, as I have already said, of three ramparts and a ditch. But two of these ramparts are on the south side of the ditch, and there is only one on the north. *The north rampart*, too, is about 30 feet distant from the edge of the ditch. A ditch or fosse is always planted on the outer or enemy's side of the fortification it is intended to strengthen, and is drawn as close to that fortification as possible. Leaving the north rampart out of consideration for the present, and taking the section of the two south ramparts and the ditch, we see that they form together an excellent defence against the north. But if we assume that all the ramparts and the intervening ditch are parts of one barrier, to be defended from its northern side, we shall, I think, be driven to confess that the position of the rebellious Brigantes behind the south rampart, or behind the one nearest the ditch, was an exceedingly advantageous position. In addition to the very frequent advantage of rising ground on their side, they had two ramparts behind which to range their forces, whilst the Romans had only one, and this more defensible position had been provided for them by the Romans themselves.

The difficulty which the section of the vallum offers to the Hadrianic theory of the object of that structure has been felt by the advocates of this theory. In an important passage in the first and second editions of *The Roman Wall*, which has been omitted in the third, the writer says :

"Whatever we may conceive to have been the design of the vallum, the peculiarity of its form will excite the attention of the enquirer, *though probably without his arriving at any satisfactory explanation*. Supposing, according to the common theory, that the vallum was an independent fortification, erected long before the wall, to resist a northern foe, why was not the ditch, as in the case of the stone-wall, drawn along the northern edge of the northern agger? I cannot supply an answer. *A similar difficulty, however* [say, rather, a twofold greater one], *meets us on the supposition that it was meant to guard against attack from the other side*. Again, what part did the smaller rampart, on the south edge of the fosse, perform? Possibly it may have been intended as a foot-hold for the soldiers when fighting on this platform against the revolted Britons south of the barrier."¹ (1st ed., p. 54-5 ; 2nd ed., p. 43.)

¹ A passage in Spartianus, to be quoted and discussed in a later part of this article, mentions a palisade of great stakes as an essential feature in Hadrian's system of border fortification. Where would such a line of stakes be placed in the works of the vallum, but on the low rampart beside the fosse? It was doubtless to receive such a palisade that this rampart was formed, whilst the inner, or southern, rampart would be occupied, in times of attempted invasion, by the forces which defended the barrier.

By "this platform" I understand the space between the two south ramparts to be meant. The Roman soldiers would here have a foot-hold two feet high and six feet broad; but the ditch, and the north rampart behind it, would be useless, except to cut off their retreat should the Brigantes prevail. And what access had the Romans to "this platform" when attacked by the Brigantes?

It seems desirable, before finally dismissing the subject of the vallum, to consider briefly Mr. MacLauchlan's contention that its north rampart was originally a military road. He says:

"There are two points where the probability is stronger than usual that the north rampart was made originally for a way. The first is where it turns aside to avoid Down Hill, near Hunnum [*Northumb.*, lxxxvi]; and the second, opposite Chesterholm [*Northumb.*, lxxxiii]. In both these cases, it is conjectured that the turn was made, not only to avoid natural obstacles, but to connect a branch-road—not a great military way, like the north rampart, but merely an ordinary line of communication, of some such inferior construction, it is supposed, as the Roman way, anciently called Stanegate. . . . Again, in the case of Carvoran, if the [north rampart of the] vallum be considered as a road, the projection may be explained as formed to avoid a bog; but, if as a defence, it would look more like a bastion thrown out to protect it [*Northumb.*, xci]." (*Memoir, etc.*, p. 91.)

Dr. Bruce, however, considers "the view that the north agger of the vallum was Agricola's military way" exceedingly improbable. He says:

"It is but ill-adapted to such a purpose. In form and composition it does not differ from the other mounds of the vallum, and in no part of its course has it been paved (!). Besides, the other lines of the vallum run perfectly parallel with the north agger, from the one side of the island to the other. Is it likely that Hadrian, when throwing up ramparts to act as a military bulwark, would follow exactly the line which Agricola, forty years before, had marked out for the purposes of transit?" (*Roman Wall*, 3rd ed., p. 375.)

Why should he not? By so doing, he would gain all the advantage of having his own ramparts strengthened by one already existing. The assertion that in no part of its course has the north rampart been paved, is incapable of proof. Had it been said that the rampart in question is not now paved in any part of its course, safer ground would have been taken. But even then we might reply that had this rampart been originally paved, the disappearance of its pavement would be amply accounted for by the subsequent construction of the murus and of the military way, much at least of which belongs so clearly, both in date and purpose, to that

barrier. But I doubt whether the rampart in question has been examined with sufficient care, throughout its course, to determine whether it does not now bear traces of having been paved. A number of trenches dug through its best preserved portions might reveal unexpected evidence.

That the north rampart of the vallum in many places has the appearance of a military road no one who has traversed it can doubt; and if the reader who has done this will turn to plates 5, 6, 13, 14, 15, and 16 of Stukeley's *Itinerarium Curiosum* (vol. ii) he will find that Roman roads in other parts of Britain have borne a very marked resemblance to this rampart of our vallum.

That the vallum was regarded as a completed work before the construction of the murus was thought of is rendered probable by a comparison between it and the Roman wall of Germany—the Pfahl-graben—the best account of which, in English at least, is a paper contributed by Dr. Thomas Hodgkin to the ninth volume of the *Archæologia Æliana*. The writer of that paper says:

“Hadrian is spoken of as the great developer of this scheme of defence at various times and in various places, and, upon the whole, the German antiquaries are probably warranted . . . in attributing to that emperor more than to any other single name the construction of the *Limes Transdanubianus et Transrhenanus*. I say ‘more than to any other single name’ because it seems to me most probable that neither the German wall nor the British was entirely the work of one man. The German camps, some of them at any rate, had probably been in existence for a hundred years or more.” (*Arch. Æl.*, ix, p. 157.)

—Just, I would add, as the Scottish camps had been in existence sixty years when the wall of Antonine was built, and just as the English camps had existed forty years when Hadrian constructed his vallum.

We have no space in which to discuss the features of the German wall, but the verdict passed upon it by Dr. Hodgkin, who has seen many parts of it, and has made himself conversant with almost everything about it in the archæological literature of Germany, is important to our purpose. He tells us that—

“It is clear that this *limes* is far more nearly related to our *vallum* and to Graham’s Dyke in Scotland (the wall of Antoninus) than to the Northumbrian *murus*. . . . In a word, it corresponds to the *vallum*, not to the *murus*, in our Northumbrian system of fortifications” (pp. 151, 80).

I cannot, perhaps, better close this part of my argument than by quoting the words of Horsley:

“I have already hinted”, he says, “what I take to have been the case,

namely, that what is now called Hadrian's north agger was the most ancient military way leading from station to station; and that Hadrian's work, which was after this, was guided and limited by it, as it keeps a constant parallelism to it. The north agger, considered as such a military way, is, as far as I can judge, conducted according to the Roman art and rules in every part of it. . . . In a word, the north agger, or old military way, keeps just such a course, and runs through such grounds, as one would expect such a Roman way should do. . . .

"I see no circumstances in the two works of Severus's wall and Hadrian's vallum that argue them to be done at the same time, or to have any necessary relation one to the other. The constant parallelism of the north agger, the ditch, and the two southern aggers of Hadrian's work, is a sure argument of their mutual relation. But this parallelism does not hold in the wall of Severus. Where they are most distant there are no visible branches of any military way leading from the one to the other, whereby the communication between them might be more easily preserved. In some places there is a morass between the two walls, which must make a retreat from one wall to the other inconvenient, and is improper for a body of men to stand on. . . .

"If Hadrian's work is supposed to have been designed for a defence against an attack from the south, difficulties of the same kind will arise, perhaps not easy to be removed. For sometimes the advantageous ground is left on the south, where it might easily have been otherwise ordered. . . . In one place the vallum runs between higher grounds on each side. Besides, if it was designed against an enemy from the south, the ditch is on the wrong side, being to the north of the two ramparts. . . . I don't see that there was any occasion to draw such a line of defence in order to prevent their making an attack upon the stations, for they are stronger on all sides than this vallum." (*Britannia Romana*, pp. 125-6.)

J. R. BOYLE.

(*To be continued.*)

THE FINN-MEN OF BRITAIN.

II.

WHEN the twelfth-century Norseman, Sigurd Slembe, with his twenty followers, spent a whole winter with the Lapps or Finns, as stated in the *Heimskringla* (Saga XIV), it is evident that the two sets of men were in intimate association. Their life at that time is thus described in Sigurd's song :

"In the Lapland tent
Brave days we spent,
Under the grey birch tree ;
In bed or on bank
We knew no rank,
And a merry crew were we.

"Good ale went round
As we sat on the ground,
Under the grey birch tree ;
And up with the smoke
Flew laugh and joke,
And a merry crew were we."

It was at that time, also, that the Lapps made for Sigurd those "skin-sewed Fin-boats", in which he and his party voyaged southward in spring. In these accounts there is no mention made of the Lapp or Finn women, but their presence there must certainly be taken for granted. And there is no reason for supposing that they were less friendly to their guests than the Finn men were. There are evidences, indeed, that the Ugrians and the non-Ugrians of Scandinavia, of either sex, were on a friendly footing two centuries before Sigurd Slembe's day. When Eric, the son of Harald Haarfager, was in Lapland on one occasion, he there found his future wife, Gunhild, living in a hut with "two of the most knowing Laplanders in all Finmark". She had come there, she said, "to learn Lapland-art," in which these two Lapps were deeply versed. The way in which she entrapped her hosts, and went off with Eric, is described in the Saga (Harald Haarfager's, chap. xxxiv), and it argues something for Eric's magnanimity or in-

difference that he chose this lady to be his bride. However, the point is that in Gunhild we have a presumably non-Ugrian woman, living in the most friendly way with a couple of Lapp "magicians".

Again, we find Harald Haarfager himself actually marrying a Finn woman. We are told (chap. xxv of his *Saga*) how, one winter, when Harald was moving about Upland "in guest-quarters", he was induced by "the Fin Svase", who announced himself to the king's followers as "the Fin¹ whose hut the King had promised to visit", to not only fulfil the said promise, but then and there to marry Snaefrid, the daughter of the Finn. Whether he took this step by reason of the beauty of the Finn girl, or of the strength of the mead which she poured out to him, or of the "magic" which she and her father exercised upon him, is a matter of little moment. The fact remains that she became his queen, and in course of time bore to him four sons: Sigurd Hrise, Halfdan Haleg, Gudrod Liome, and Rognvald Rettelbeine: who, consequently, were half-bred Finns—that is, assuming that Harald himself was of pure non-Ugrian blood.

These four sons of Harald's Finn wife are subsequently to be met with in this *Saga*; which tells how "they grew up to be very clever men, very expert in all exercises". When Harald was fifty years of age, he gave to them, as to his other sons, "the kingly title and dignity", assigning to them, as their portion of his kingdom, the territories of "Ringerike, Hadeland, Thoten, and the lands thereto belonging". But one of them, Halfdan, did not live to attain this dignity. Several years before, he, like Harald's many other sons, had resented his exclusion from place and dignity, and the advancement of mere "earls" instead; "for they [Harald's sons] thought earls were of inferior birth to them." Consequently, Halfdan and his brother Gudrod "set off one spring with a great force, and came suddenly upon Earl Rognvald, Earl of Möre, and surrounded the house in which he was, and burnt him and sixty men in it." Then, leaving his brother in temporary possession of that earldom, "Halfdan took three long-ships, and fitted them out, and sailed into the West Sea." The Earl of Orkney at that time was Einar ("Turf" Einar), and on Halfdan's unexpected appearance he fled. For six months the Finn woman's son ruled over Orkney.

¹ In the edition of 1844, the word "Laplander" is used instead of "Fin" in these two instances, as also in the following chapter, where "the cunning of the Fin woman" is referred to. But the admirable edition of the present year employs "Fin" in each case. Whatever may have been the original distinction between "Fin" or "Finn" and "Lapp", it is evident that these two terms have very often been used indiscriminately, from an early period.

But in the autumn, Einar returned, and, "after a short battle", totally defeated and put to flight Halfdan and his followers. "Einar and his men lay all night without tents, and when it was light in the morning they searched the whole island, and killed every man they could lay hold of. Then Einar said: 'What is that I see upon the Isle of Ronaldsha?'¹ Is it a man or a bird? Sometimes it raises itself up, and sometimes lies down again.' They went to it, and found it was Halfdan Haaleg, and took him prisoner." Einar thereupon killed Halfdan, and he and his men raised a mound of stones and gravel over the corpse; which mound, if not yet opened, will no doubt disclose to some modern craniologist the exact ethnological status of this semi-Finn.²

With regard to another brother of Halfdan's, Rognvald Rettilbeine, it is stated that he ruled over Hadeland, and became famous for his skill in witchcraft, in which he was no doubt instructed by his Lapp relatives. This, indeed, was the cause of his death. For, at the instigation of their common father, his half-brother Eric (Bloody-axe) "burned his brother Rognvald in a house along with eighty other warlocks", on account of these same alleged malpractices.

These are only a few recorded instances, which reveal the Finns and the non-Finns as sometimes closely allied not only by association, but by blood. But from them it may be inferred that many other intermarriages between the two races took place, and that the Finns, although eventually conquered as a distinct people, were frequently men of rank and importance among the Scandinavians of eight or nine centuries ago. As an instance of a Finn occupying an official position (certainly much inferior to that of the semi-Finnish kings of Ringerike, Hadeland, and Thoten), we

¹ It is stated of Einar that, although "he was ugly, and blind of an eye", he was "yet very sharp-sighted withal".

² Mr. John R. Tudor, in his very interesting book on *The Orkneys and Shetland* (London, 1883), indicates (p. 364) a certain district in the island of North Ronaldshay as the scene of Halfdan's death; and suggests that one of "three curious ridges or mounds" is probably that raised over Halfdan's body. But, of course, there is plenty of room for conjecture in the whole story. Indeed, whether the island on which Einar detected Halfdan was North Ronaldshay or South Ronaldshay, it was impossible for Einar to see anybody hiding there, if the battle was fought on the mainland of Orkney. It is possible, however, that the scene of the battle was the small island of Burray, and that Halfdan took refuge in *South* Ronaldshay, across the narrow strait. (It may be noted, although this is a mere coincidence, that it was in the church of Burray that one of the Finn kayaks was preserved, at the close of the seventeenth century; according to the testimony of Wallace.)

have the "Finn Sauda-Ulfsson", who appears as "engaged in drawing in King Inge's rents and duties" at Viken, Norway, in the twelfth century (*Heimskringla*, Saga XIV, chap. vii). And a certain notable Ketill flat-nose,¹ or Ketill Finn, whose memory is doubtless embalmed in Ketill's-sæter (now Kettlester), in the island of Yell, Shetland, was clearly of Finn blood. When he, and such as he—the semi-Ugrian sons of Harald, for example—held sway in Shetland and Orkney, and when men and women of either race occasionally, perhaps frequently, lived together, a state of things existed that closely resembled that described in Mr. Karl Blind's Shetlandic traditions—when "Finns came ow'r fa Norraway" in their "skin-sewed Finn-boats", and practised magic and witchcraft, and domineered over the people of the northern islands.

Of course, it is impossible to say what proportion the Finn blood bore to the other. Yet it is quite evident that the Finns, while often at war with the race that overcame them, were also frequently their allies, and that the two peoples became to some extent blended in blood. Consequently, when one discovers among modern British people physical traces of a race "not unlike the modern Eskimo", in localities famed as the scene of many a Scandinavian raid, these traces may reasonably be attributed to those very inroads.

These references bear specially upon those Finns who "came ow'r fa Norraway" to the islands of Shetland and Orkney. But if the assumption be correct that many of the Finns who landed in Shetland and fished in Shetlandic waters came thither direct from the Hebrides, it is to be presumed that Gaelic as well as English tradition has something to say regarding them. And as there are several words in use in Shetland which are also in use among West Highlanders,² it is not unlikely that these people may

¹ Mentioned, for example, in Skene's *Celtic Scotland*, i, 311-312. It is not out of place to refer here to a Mongoloid race of "Flat-noses" of whom Mr. Howorth speaks. These are the Nogais, who are known as "Manguts"; the word *Mangut*, or *Mangutah*, being "merely an appellative, meaning flat-nosed". "Dr. Clarke says of them: 'They are a very different people from the Tartars of the Crimea, and may be instantly distinguished by their diminutive form, and the dark copper colour of their complexion, sometimes almost black. They have a remarkable resemblance to the Laplanders, although their dress and manner has a more savage character.' Pallas enlarges also upon their specially Mongolian features. Klaproth says: 'Of all the Tartar tribes that I have seen, the Nogais bear by far the strongest resemblance in features and figure to the Mongols.'" (Howorth's *History of the Mongols*, part ii, p. 2, and part iii, p. 71.)

² Such as *roo* and *mål* (each used to denote a headland); *skerrie*, a reef; *couthie*, the "cuddy" or coal-fish, and *broch*; all of which are found in Gaelic as *ru* (*rudha*), *maol*, *sgeir*, *cudan*, and *brog*.

may be known in the West Highlands by the same name as in Shetland.

It is quite clear that Highland tradition does bear testimony to the former existence of a special race or caste of people known by a name which resembles that of the Finns so closely that it may reasonably be regarded as only a variant of "Finn". In a certain charter of Alexander II of Scotland (A.D. 1214-49), reference is made to a well which is known in Gaelic as *Tuber na Feinn*, *Feinne*, or *Feyne*; and an old gloss (date unknown) explains that this term signifies "the Well of the grett or kempis men callit Fenis".¹ Or, in more modern English, "The Well of the great men or champions called *Feens*, *Fenns*, *Feenies*, or *Fennies*".² Here, then, we have record of a certain race of "kempies" or fighters, who were known in English as *Feens*, etc., and in Gaelic as the *Feinne*. One does not require to know much of Gaelic tradition—one need not know anything of it—to be well aware of the fact that that legendary lore is fairly alive with stories of the "Feinne", whatever may have been the ethnological position of the caste thus named. And, just as in modern Shetland we have people proclaiming with pride their descent from the *Finns*, so have we West Highlanders and Hebrideans boasting that the *Feinne* were among their forefathers. Just as Mr. Karl Blind met with a modern Shetland woman who asserted that she was "fifth from da Finns", so 'told the late Mr. J. F. Campbell, in 1871, converse with a Skyeman, Donald MacDonald, styled *Na Feinne*³—that is, "of the Feens". If the "Feinne" of Gaelic story are really the same people as the "Finns" of Shetlandic tradition, it will not be for lack of statements made regarding them if we do not learn a great deal more about these people through Gaelic channels.

Without either hastily accepting or condemning this hypothetical identification, let us look a little further into the circumstances of the Gaelic *Feinne*. And it may be as well first to decide upon an English equivalent of this Gaelic plural. Mr. J. F. Campbell states that the singular is *Fiann*; but, even when writing in English, he prefers to adhere to the Gaelic form of the plural—

¹ See p. lxxx of Dr. Skene's Introduction to *The Dean of Lismore's Book*, Edinburgh, 1862.

² Perhaps the old Scotch termination "is" ought not to be modernised into a separate syllable, as, whatever the force once given to it, that termination represents the modern plural and possessive "s". But if the "Fenis" of the gloss was dissyllabic, it has an equivalent in Shetland in the alternative "Finny", sometimes used instead of "Finn".

³ See *Leabhar na Feinne*, London, 1872, p. iv.

thus, "the Feinn" or "the Feinne". However, both Dr. Skene and another writer (the late Rev. J. G. Campbell, Tíree), have Englished this into "the Fians". This approaches so closely to the marginal "Fenis" of the old charter of Alexander II, that we may take "the Feens" as a good enough modern English equivalent for the Gaelic plural. (For the vowels in *Fians* and *Feinne* receive the old or Continental pronunciation, these words having the sound of "Feeans" and "Fane", or "Fayny", according to modern English spelling.) In order, therefore, to avoid the confusion that might arise from Englishing "the Feinne" into "the Finns" (although we are tacitly assuming, in the meantime, that the latter really expresses the ethnological position of the former), let us refer to "the Feinne" of Gaelic story as "the Feens".¹

So lately as the latter part of the seventeenth century, certain districts of Scotland were recognised as specially "the land of the Feinne". Dr. Skene, on the page which tells us of the *Tobar na Feinne*, or Well of the Feens, states that Kirke (the Rev. Robert Kirke, minister of Balquhiddy, in Perthshire), in his Psalter, which was published in 1684, refers to the territory stretching from Loch Linnhe, north-west to, and inclusive of, the Outer Hebrides,² as "the generous land of the Feinne".

"The land of the Feens", therefore, according to this Scotch writer of the seventeenth century, embraced the Outer Hebrides and a certain portion of the opposite mainland, known in the Highlands as "the rough bounds". It is thus evident at the outset that we do not obviously make a false start in assuming that the *Feens* of Gaelic tradition ought to be regarded as forming a section of the *Finns* who visited Shetland in the seventeenth century. In 1684 Kirke regarded the Hebrides as the land of the Feens; in 1688 Wallace records the occasional arrival of Finns or Finn-men on the coasts of Orkney and Shetland. And we have already seen that skin kayaks, such as those which bore the Finn visitors to the islands of the north-east, were employed at about the same period by inhabitants of the Hebrides. Certain sections of the Hebrideans

¹ It may be added, that while Dr. Skene frequently speaks of "the Fians", and at other times of "the Feinne", he occasionally refers to "the Fenians". But, as this term has been recently usurped by a quasi-political faction, and as it is, moreover, less accurate than the other, we may at once reject it. The compound "Fingalian" has also little to recommend it.

² "The Rough-bounds (*Garbhcrioch*) and the Western Isles" is the expression used. The former term denoted that portion of the mainland between Loch Linnhe and Glenelg. Whether the Island of Skye ought to be included as one of the "Western Isles" is not quite clear.

are recorded in history as making warlike descents upon the fisheries of Orkney and Shetland. And these Hebrideans dwelt in "the land of the Feens".

But the seventeenth century is much too recent a date for studying the Gaelic accounts of the Feens. These accounts go back to the period when Gaelic was peculiarly associated with what seems to have been its earliest home in the British Islands—Ireland. That they also relate to the more recent period of the Irish or Gaelic settlements in Scotland is manifest. But they are substantially Gaelic (*i.e.*, Irish), and they deal with events which cannot be limited to the time of the Irish invasions of Scotland; and they relate to localities which are not merely British, but European.

"Who were the *Feens* of tradition, and to what country and period are they to be assigned?" is the question asked by one of the most learned of the authorities from whom these statements are obtained.¹ And his answer, after due consideration, is, that "we may fairly infer that they were of the population who immediately preceded the Scots [Gael] in Erin [Ireland] and in Alban [Scotland, north of the Forth and Clyde], and that they belong to that period in the history of both countries before a political separation had taken place between them, when they were viewed as parts of one territory, though physically separated, and when a free and unrestrained intercourse took place between them; when race, and not territory, was the great bond of association, and the movements of their respective populations from one country to the other were not restrained by any feeling of national separation."²

Distinct and important as this announcement is, it requires still further consideration. Our guide in this question has shown us that in such modern times as the seventeenth century, the Feens of Scotland were restricted to a small corner of the West Highlands and to the Hebrides; which territory was so far associated with them that an intelligent writer of that century spoke of it as the land of the Feens. But Dr. Skene points also to a much earlier period, when the Feens inhabited, if they did not possess and exclusively occupy, the whole of Ireland and Irish-Scotland. And he indicates further that they had dwelt in these districts

¹ Dr. Skene, p. lxiv of his Introduction to the *Dean of Lismore's Book*. (Here, as elsewhere, I take the liberty of substituting *Feens* for the Gaelic plural *Feinne*.)

² *Op. cit.*, Introduction, p. lxxviii.

before the advent of the Milesians (or Gaels). More than that, he shows us that the lands in which they lived included a portion of the continent of Europe.

In opposition to the theory manufactured by the Irish historians, that the Feens were "a standing body of Milesian militia, having peculiar privileges and strange customs", Dr. Skene holds the conviction that, "when looked at a little more closely", they "assume the features of a distinct race."¹ As a proof of this, he quotes three verses from an old poem on the Battle of Gabhra (or *Gawra*, as the more softened pronunciation has it). This battle of *Gawra* is said to have been fought in Ireland, on the border of the counties of Meath and Dublin, and it is placed by some in the third century A.D. It appears to have been the outcome of the resolution made by the High King of Ireland, Cormac Mac Art, to renounce for ever the tributary position which he and other kings occupied towards their over-lords, the Feens. The Irish monarch is said to have aimed at the complete extermination of the race in one district at least; to have "Great Alvin [apparently the modern Allen, near Dublin] cleared of the Feens".² At any rate, whatever its position in time and place, this battle clearly marks a crisis in the history of that latter race. For to them the battle of *Gawra* was a complete and crushing defeat; and thereafter their suzerainty was ended. "The kings did all own our sway till the battle of *Gaura* was fought", sings the bard of the Feens, "but since that horrid slaughter no tribute nor tax we've raised." The chroniclers state that the leader and an immense number of his warriors were killed, and only two thousand of the Feens of Ireland were left alive when the battle was over. And their bard sings thus:

"Fiercely and bravely we fought,
That fight, the fight of *Gaura*;
Then did fall our noble Feinn,
Sole to sole with Ireland's kings."³

But the Feenian army here engaged did not only consist of the Feens of Ireland; and this, indeed, is the reason why attention is now drawn to this battle. It is in regarding the battle of *Gawra* that we recognise the force of Dr. Skene's contention, that however the Feens may in later times have become restricted to this or that locality, they at one time formed a very widely spread *race*, the

¹ *Op. cit.*, Intro., pp. lxxiii-lxxiv.

² *Op. cit.*, p. 36.

³ For the above references, see pp. 36, 37, and 40 of the *Dean of Lismore's Book*.

various divisions of which were ready to hasten to the aid of any portion of this great confederacy in time of danger. Whether Dr. Skene is precisely correct in stating that "race, and not territory, was the great bond of association", is a mere question of words. Because the Gaelic traditions emphatically show that although Ireland and other neighbouring lands were occupied by people of non-Feenic race, who were governed by their own kings, yet, as these kings were *themselves* subject to the Feens, who drew tribute from them, the real owners of these various territories were the powerful though scattered over-lords, and not the races that were under their sway.¹ Mr. J. F. Campbell also states that the Feenic king was not distinguished by any *territorial* title: "always 'Rìgh na Fìinne or Féinne'" (*West Highland Tales*, I, xiii). And in the pedigree which he gives on page 34 of his *Leabhar na Feinne*, and which was compiled by a good archæologist, the title given to three successive generations of the "royal family" of the Irish Feens is "General of the Feens" of Ireland; not "King of Ireland" itself.

This battle of Gawra, then, which seems to mark the period when the great Feenic confederacy was on the point of breaking up, was brought about by the evident resolve of the non-Feenic population of Ireland to throw off for ever this intolerable yoke. And the three verses which Dr. Skene extracts from the poem descriptive of the battle disclose to us that other sections of the Feenic confederacy had come to the help of that division which was resident in Ireland. The poem is supposed to be sung by a Feen of Ireland; and he states that

"The bands of the Feens of Alban,
And the supreme King of Britain,
Belonging to the order of the Feens of Alban,
Joined us in that battle.

"The Feens of Lochlin were powerful,
From the chief to the leader of nine men,
They mustered along with us
To share in the struggle.

* * * * *

"Boinne, the son of Breacal, exclaimed,
With quickness, fierceness, and valour,—
'I and the Feens of Britain
Will be with Oscar of Emhain.'"

¹ Just as modern India is *British* India, although it is almost exclusively occupied by native races. (In this instance, of course, the position of *native* and *alien* is precisely the reverse from that which this "Feen" empire seems to denote.)

"There was thus in this battle", says Dr. Skene, "besides Feens of Ireland, Feens of Alban, Britain, and Lochlan."¹ Alban, he explains, denoted the whole of Scotland lying to the north of the Forth and Clyde. Britain, he states in this place, was South-Western Scotland. But elsewhere² he tells us that "Britain" signified "either Wales, or England and Wales together"; and again,³ that that term included "England, Scotland, and Wales". At the very least, then, it denoted a part of Great Britain, then inhabited—not necessarily to the exclusion of other races—by Feens.

These two names, "Alban" and "Britain", do not, however, take us outside of the British Isles. But the third term, "Lochlan", does. "Lochlan", says our guide, "was the north of Germany, extending from the Rhine to the Elbe." And the Feens of that territory, the poem tells us, "from the chief to the leader of nine men", "mustered along with us [the Feens of Ireland] to share in the struggle", on this fateful day of Gawra.

Why Dr. Skene should limit "Lochlan" to these dimensions is not made quite clear. For Norway, Sweden, and Denmark constituted the "Lochlan" chiefly known to Gaelic writers. However, he seems to be of opinion that the term was "transferred" to Scandinavia in the ninth century, and that previously (as, for example, when the battle of Gawra was fought) it peculiarly denoted the more southern territory. If he is right in this, we cannot assume the Lochlan contingent as including the Feens of Norway. On the other hand, there does not seem to be any strong reason for believing that, at the date of Gawra, "Lochlan" did not take in the whole of Scandinavia, as in the ninth century and afterwards. It is at least noteworthy, in this connection, that in the pedigree previously referred to,⁴ the mother of the ruler of the Feens of Ireland, when the battle of Gawra was fought, is stated to have been a *Finland* woman. Quite apart from the assumed identity of *Feen* and *Finn*, this indicates a kinship that was not limited even by the river Elbe.⁵

¹ *Dean of Lismore's Book*, p. lxxv. The spelling is here slightly modified.

² *Op. cit.*, p. 8, note 1.

³ *Op. cit.*, p. 49, note.

⁴ *Leabhar na Feinne*, p. 34.

⁵ The Gaelic traditions have a good deal to say regarding a race of searovers, styled *Fomorians*; which word is by some believed to be a latinized form of a Gaelic term denoting a seafaring people. As it is not improbable that this may be simply another name for the people now under consideration, the following is worth citing here: "That those adventurers whom our writers call Fomorians, have arrived hither in multitudes from that country whence the

But really the identity of *Feen* and *Finn* seems tolerably clear. Indeed, a contemporary writer,¹ who has studied ancient Ireland and its "*Feinne*" from his own point of view, appears to regard this identity as a thing perfectly manifest. And when, as tending to confirm this opinion, he embellishes his pages with several illustrations from scientific authorities in modern Finland, in which the ancient forms of art and dress are seen, it is plain that these designs are the same as those which are strongly associated with those portions of Scotland which were once known as The Land of the Feens.

Therefore, it appears probable that the "*Feinne*" of Lochlan, that is, of the country lying between the Rhine and the Elbe, who assisted their kindred in Ireland at the battle of Gawra, were simply the Finns of that territory. And that, consequently, that battle belongs to a period when the Mongoloid people, instead of being cut up, as now, into small detachments here and there, or amalgamated with other races, held a very distinct and important position throughout a considerable area of Europe.

However, this identity of "*Feen*" with "*Finn*" may not appear to some people as even a probability, without a fuller investigation into the circumstances of the people known to Gaelic tradition as the *Feinne*. It may therefore be desirable to continue to refer to the "*Finns*" of Gaelic folk-lore by the name of "*Feens*".

"The Feens, then, belonged to the pre-Milesian races, and were connected, not only with Ireland, but likewise with Northern and Central Scotland, England and Wales, and the territory lying between the Rhine and the Elbe.² Now, there are just two people mentioned in the Irish records who had settlements in Ireland, and who yet were connected with Great Britain and the region between the Rhine and the Elbe. These were the people termed the Tuatha De Danann, and the Cruithné." So says the learned annotator of *The Dean of Lismore's Book*.³

These two last-named races, we are told, are both traditionally Danes, Swedes, and Norwegians came, is a circumstance that may be collected from this account, that the father-in-law of Tuathal is said, in the genealogy of the kings of Ireland, to have been king of the Fomorians of Finland." (O'Flaherty's *Ogygia*, Hely's translation, Dublin, 1793, vol. i, p. 19.)

¹ Mr. Charles de Kay, in the course of several learned articles on early life in Ireland, contributed to *The Century Magazine* during the present year.

² It is to be remembered that "*Lochlan*", the term used to denote the territory last named, was ultimately applied to the whole of Scandinavia, and may have been used in its widest sense at the period here referred to.

³ *Introduction*, p. lxxvi. In the above, I have again taken the liberty of modifying the various designations.

brought from the Elbe and Rhine districts to Ireland and Scotland, and both are eventually subdued by the later-arriving Milesian Scots. The period given for the Milesian conquest of the Cruithné of Scotland, is the ninth century of the Christian era.

Leaving the "Tuatha De Danann" out of the question, in the meantime, let us look at the contemporary, and probably kindred, "Cruithné". The Cruithné, Cruithneach, or Cruithnigh, are unquestionably deserving of study, for Dr. Skene has shown us¹ that this is merely another name for those people whom history chiefly knows as "the Picts". The traditional "Feens", therefore, are to be identified with the historical "Picts".

Now, although these people are, as we have just seen, believed to have come from the Continental country of "Lochlan" (Scandinavia, in the largest acceptation of that term, or, in its most restricted sense, the region lying between the Rhine and the Elbe), and although there is every reason to believe that they spread themselves all over the British Isles, yet they seem—regarded as "Picts"—to be chiefly associated with North Britain. Their memory is still preserved, topographically, by the name of *Pentland* (formerly *Petland* or *Pehlant*, and *Pictland*), which is borne by the stormy firth separating the Orkneys from Caithness, and also by the range of hills lying to the south of Edinburgh. Both of these names are unquestionably derived from the time when there was a "land of the Picts" in either of these neighbourhoods. But the Picts, as such, are remembered all over Scotland, in history and in tradition. It is chiefly in connection with Ireland that they are spoken of as Cruithné.

If the "Feens" of tradition were *Cruithné*, or *Picts*, it is evident that whatever is known with regard to the history, customs, appearance, and language of the Picts will help us to decide as to whether the *Feens* were really one with the *Finns* of history, ethnology, and tradition. This, as already remarked, on general grounds, seems very probable. But, when a very able historian assures us that the historical Cruithné or Picts must certainly be at least classed with the Feens of tradition, if these three terms do not actually include one people, we are enabled, by proceeding upon this assumption, to obtain further proofs in corroboration of this belief.

Whether regarded as Feens or as Picts, these people, we are informed, had settlements throughout the British Isles during the

¹ *Celtic Scotland*, vol. i, p. 131; vol. iii, chap. iii, etc. See also his *Chronicles of the Picts and Scots*.

earlier centuries of the Christian era, and the country of their origin was Northern Germany (or, more vaguely, Scandinavia); in which country large sections of their kindred continued to dwell, and to maintain a system of confederacy with the Western or British section long after the latter had settled in their new home. This, at any rate, when viewed as Feens.

On the other hand, such a writer as Mr. H. Howorth demonstrates that, during the same period, the Mongoloid races formed a most important, and in some places a preponderating, portion of the inhabitants of the countries of Northern Europe. But, during that period, these Mongolian races have—he points out—been subjected to an unceasing process of expulsion from their neighbours on the south and south-east. If any race, therefore, arrived in the British Islands from the neighbourhood of the Baltic in the centuries immediately preceding or following the birth of Christ, the probability is that that race belonged to one division or another of these dispossessed Ugrian people.

If this were so—if the Cruithné or Picts, who came to Britain from the Baltic lands, were one with, or closely akin to, the Finns and Lapps—their characteristics must have been those of such people. For example, their religious beliefs. Now, one cannot read Dr. Skene's references to the heathen religion of the Cruithné without seeing that it strongly resembles that of the Lapps and Finns.¹ Without quoting these references in detail, it may be pointed out that the power of bringing on a snowstorm and darkness, and unfavourable winds, was among the mysteries of the Pictish priests. And this gift of commanding the elements was peculiarly associated with the Finns and Lapps, as it still is with the Eskimo "sorcerers" of Greenland. "In the Middle Ages", says a writer on sorcery,² "the name of *Finn* was equivalent to sorcerer." And as the same writer observes that "the old authors often confounded the Finns with the Lapps, and when they speak of Finns, it is very difficult to know which of these two peoples they refer to" (a confusion of terms which we have already had occasion to remark), we may here use the term *Finn* to denote both divisions. Tentatively, at any rate. The actual Lapps appear to have been the most powerful magicians of all that caste. "It is proved by numerous documents", continues M. Tuchmann, "that the Finns called the Lapps sorcerers, although they themselves were reputed to be great magicians; but they regarded

¹ *Celtic Scotland*, vol. ii, pp. 108-16.

² M. J. Tuchmann, in *Mélusine*, t. iv, no. 16.

themselves as inferior to their neighbours, for they habitually said, when speaking of their most famous sorcerers : ' He is a veritable 'Lapp.'"¹ However, since "Finn" has so frequently been used to denote the whole group, and since the most recent examples of these people in the British Isles, namely, the magic-working Finns of Shetland, have borne that title, we may adhere to the practice of referring to both divisions as "Finns".

The Picts or Cruithné, therefore, practised the magic of the Finns. That is, the *Feens* practised the magic of the *Finns*.²

Again, when we look at certain weapons used by the *Feens*, a similar resemblance is visible. According to a tradition, taken down from the recital of an old Hebridean, the spears or darts of the *Feens*, which were known in Gaelic as "*tunnachan*" were of this description : " They were sticks with sharp ends made on them, and these ends burned and hardened in the fire. They [the *Feens*] used to throw them from them, and they could aim exceedingly with them, and they could drive them through a man. They used to have a bundle with them on their shoulders, and a bundle in their oxters [under their arm-pits]. I myself have seen one of them that was found in a moss, that was as though it had been hardened in the fire."³ " This, then", justly remarks Mr. Campbell, " gives the popular notion of the heroes [the *Feens*], and throws them back beyond the iron period."

While the fashion of referring to "periods" of iron, bronze, etc., is very apt to mislead (since contiguous peoples have been, and are, in different "periods" of this nature, at the same moment of time), it is at least clear from the above tradition that the most primitive form of dart was associated with the *Feens*. But, although this species of weapon is of great antiquity, it does not follow that a tradition which relates to people who employed it is

¹ Mr. Charles de Kay, in one of the valuable articles already referred to, remarks ("Woman in Early Ireland", *Century Magazine*, July 1889, p. 439) : " Although in the Kalewala the tribes of Pohjola, or the Lapps, are considered foul magicians, and ever the foe of the heroes of Kaleva, or the Finns, yet it is from Pohjola that Wainamöinen and his comrades always take their brides by force or by purchase." This quotation not only confirms the above account of M. Tuchmann, but it also illustrates the fact that even the most antagonistic races do not refrain from mixing their blood. Thus it may be seen how Lapps and Finns could eventually become almost identified. And the *Heimskringla* shows us how, in turn, this composite Finno-Lapp race could later on become blended with that of the Haralds and Sigurds of the Sagas.

² This has already been propounded by the late Mr. J. F. Campbell (*West Highland Tales*, iv, 29-30).

³ *West Highland Tales*, iii, 394-5.

necessarily of great antiquity also. Or that those javelin-men were at all "pre-historic". We have already seen that a race of people employed darts in exactly the same way when fishing—or, perhaps, more correctly, when seal-hunting—within British waters, only two hundred years ago. And the people who in this respect resembled the *Feens* of Gaelic folk-lore are themselves remembered as *Finns*.

But perhaps the readiest and surest way of obtaining something like a true conception of these legendary Feens, is to regard them from the ethnological point of view, as well, that is, as our imperfect information will allow. We shall therefore look at them in this aspect, whether considered as *Picts* or *Cruithné*, or as *Feens*.

The great hero of the Feenic legends, and the "King" or "General" of the Feens of Ireland, was the famous "Finn" or "Fionn". If the battle of Gawra was really fought in the third century, as is alleged, and if this "Fionn" was a real man, and not the type or "eponymus" of his race, then he ought to be assigned to the third century. For he is said to have been present at that battle, where his grandson was slain and the supremacy of his race destroyed. At any rate, whether he lived at that date or not, and whether he was an individual or merely a personification of his race, Fionn figures throughout the tales of these people as a very Feen of the Feens.

Now, among the many stories told of him, there is one entitled "How Fin¹ went to the Kingdom of the Big Men". It is unnecessary to give all the particulars of this tale. But Fin is pictured as starting from Dublin Bay in his little coracle (*curachan*) on his voyage to the country of the Big Men. Although he is described as "hoisting the spotted, towering sails", they cannot have been very large or very many, for the coracle was so small that "Fin was guide in her prow, helm in her stern, and tackle in her middle", and when he landed on the coast of the Big Men's country, he drew his tiny vessel, unaided, up into the dry grass, above the tide-mark. It ought to be added, however, that this coracle was an open boat, capable of holding at least four persons; as is shown on the return voyage.

After landing, Fin encountered a "big wayfarer" (*taisdealach mór*), who informed him that his king had long been in want of a dwarf (*troich*), and that Fin would suit him capitally. "He took with him Fin; but another big man (*fear mór*) came, and was

¹ So spelt in the English translation given by the Rev. John G. Campbell, minister of Tiree, in *The Scottish Celtic Review*, Glasgow, 1885, pp. 184-90.

going to take Fin from him. The two fought; but when they had torn each other's clothes, they left it to Fin to judge. He chose the first one. He took Fin with him to the palace of the king, whose worthies and high nobles assembled to see the little man (*an duine bhig*). And then and there Fin was installed as the royal dwarf.¹

In this story, then, we have the tacit admission that, not far from Fin's home at the hill of Allen, Kildare, there was a country whose inhabitants were so much taller than the race of Fin, that the latter were mere dwarfs beside them. Now, this is precisely the most striking characteristic of the kayak-using Finns of Shetlandic tradition.

The *Finns* of Shetland folk-lore are, says Mr. Karl Blind, "reckoned among the *Trows*". The king of the *Feens* was hailed in the country of the big men as a *Troich*. And these are simply two forms of the same word. *Troich* or *droich*, among Gaelic-speaking people, is softened into *trow* or *drow* among the English-speaking Shetlanders.² In both cases it signifies "dwarf".

And, just as the Shetlanders have memories of a race of small men, who, in spite of their mean stature, were a terror to the taller people, whom they oppressed and took tribute from, so have the Gaelic-speaking people a mass of legends which also tell of similar dwarfish but dreaded tyrants. The former designate their dwarfs "Finns": if the Gaelic traditions are not equally definite, they at least suggest that a caste of "Feens", who levied a tax upon the Gaelic-speaking people, were themselves dwarfs in stature. And the Highland tales abound in stories of fierce and tyrannical dwarfs.

But, if the legendary "Feens" are identical with, or closely akin to, the Picts of history, then the historical Picts must also belong to this stunted Eskimo-like race. Let us look at the people called "Picts".

And, first of all, since the word "Pict" is admittedly the result

¹ Referring to the component parts of Fin's army on a certain occasion, Mr. Charles de Kay remarks ("Early Heroes of Ireland", *Century Magazine*, June 1889, p. 200): "The battalion of 'middle-sized men' and that of 'small men' we may understand as recruited from the true hunter and fisher tribes, who gave the name Fenian to the army itself, and Fion to the folk-hero."

² *Trow* is the favourite form among the Shetlanders; but other forms are given by Edmondston in his *Glossary*, such as *drow*, *troll*, *troil*, *troilya*, and *trolld*. The Shetland terms are, therefore, also variants of the Scandinavian *troll*, following a common Scotch tendency, which modifies *boll*, *knoll*, *poll*, *roll*, etc., into *bow*, *know*, *pow*, *row*, etc. (the vowel sound being as in *now*). But whichever form may be the oldest, it is manifest that *trow* or *drow*, and *troich* or *droich*, are radically one.

of a pun or a misapprehension on the part of Latin-speaking people, it may be as well to discard that special spelling. The forms which the word appears to have most commonly taken in the mouths of the country-people of Scotland are *Pik*, *Pech*, *Pecht*, and *Pelt* (the *ch* being of course pronounced as in German). Doubtless, other forms might be adduced; but perhaps the best compromise is *Pecht*. What, then, are the accounts given with regard to the stature of the Pechts?

The question is practically answered at once in considering the nature of the dwellings that the traditions of Scotland unanimously assign to these people.

"The only tradition which I heard current on the subject of the former inhabitants of the country", says a writer on Shetland,¹ "was, that the remains of old dwellings were Pechts' houses, and that those who lived in them were little men." And, in reporting to the Anthropological Society of London the result of an archaeological tour in Shetland, Dr. James Hunt² remarks of such "old dwellings"—"These remains are called 'Pights' or Picts' houses.' Mr. Umfray [a local archaeologist] surmises that they were originally 'pights', or dwarfs' houses. Dwarfs, in this locality, are still called *pechts*."³ And the present writer, when visiting a "Pict's house" three or four miles north of the place just spoken of, and which had also been inspected by Dr. Hunt, obtained similar testimony. This place is known as Saffester, or Seffister, and its antiquarian features consist of the remains of a chambered tumulus and a separate subterranean gallery. The latter is referred to by one writer as a "Pict's house", although it is only a passage. As, however, local tradition alleges that it leads to the chambered mound, the name may be correct enough. Now, this tumulus was opened fifty or more years ago by the parish minister.⁴ And an old man, who was then a boy, informed the writer that the entrance was effected by what he and his boy companions had always called "the *trow's* door". Another similar experience of the writer's yields a like result. Near Hamna Voe, at the south end of the island of Yell, there is a small loch and islet, with the remains of a "broch", the loch being known as "the loch of Kettlester". The

¹ Rev. J. Russell, *Three Years in Shetland*. Paisley and London, 1887, pp. 135-6.

² See the Society's *Memoirs*, 1865-6, vol. ii, pp. 294-338.

³ The spelling *pight*, which Dr. Hunt uses above, must clearly represent the guttural and vowel sound of *licht*, *nicht*, *dight*, etc., in "broad Scotch". Without this caution, the reader would naturally infer the sound of *pite*.

⁴ Rev. J. Bryden: see *Anthrop. Soc. Mem.*, *ut supra*.

"broch" that once stood there (for the ruins no longer retain their original shape) was built by "the Pechts", said the intelligent lad (a native of the district) who was the writer's guide, and these Pechts he described as very small people.¹

The popular Shetland notions regarding the Pechts are again repeated by a lady writer, who has the advantage of being herself a Shetlander²: "The first folks that ever were in our isles were the Picts. . . . They had no ships, only small boats. . . . They were very small [people]." Indeed, so much has their small stature been impressed upon the popular memory, that, as we have seen, "dwarfs, in this locality, are still called *pechts*". Nor is it only in Shetland that this word has such a meaning. In Aberdeenshire *picht* denotes a dwarfish person, and Dr. Jamieson, in recording the fact,³ suggests its connection with "the *pichts* or *pechts*, whom the vulgar view as a race of pigmies." In the south of Scotland, also, this signification appears to prevail; for the Ettrick Shepherd, in the *Noctes Ambrosianæ*, employs "pegh" as an everyday synonym for "dwarf". In point of fact, although it has just been stated that dwarfs "are still called *pechts*" in Shetland, because of the small size of the race so known to history, it is really a question whether the historical people did not so become historically remembered *because* a pre-existing word fitly described their dwarfish stature. But this etymological point is of little importance here.

Although Shetland has been chiefly considered in these recent remarks, it will be seen that the popular belief regarding the stature of the Pechts is apparently common to the whole of Scotland. Dr. Jamieson evidently thought so when he referred to "the Pichts, or Pechts, whom the vulgar view as a race of pigmies". And he does not stand alone. "Throughout Scotland", says another writer, "the vulgar account is 'that the *Pechs* were unco wee bodies, but terrible strang'; that is, that they were of very small stature, but of

¹ Close to Kettlester there is a noted haunt of the "trows", which bears the name of *Houlland*. With this may be compared *Troil-Houlland*, which adjoins Seffister, of "trow" memory. This very common Shetland termination "ster" or "setter" is the Icelandic *setr*, a dwelling; and these two names resolve themselves respectively into dwellings of *Kettle* and *Seffi*. The former name at once recalls the ninth century *Ketil Flat-nose* of the Sagas; and this "setr", still associated with dwarfs (otherwise *trows* or *pechts*), may have been one of his dwellings.

² Mrs. Saxby, in "Folk-lore from Unst, Shetland" (Part v), contributed to *The Leisure Hour*, 1880.

³ *Scottish Dictionary* (Supplement), s. v. "Picht."

prodigious strength."¹ "Long ago", quotes the late Robert Chambers,² and his quotation also applies to the whole of Scotland, "there were people in this country called the Pechs; short, wee men they were,"—and so on.

Enough has been said to show that the ideas held by the "vulgar" (whose traditions, once contemptuously rejected by scholars, are nowadays being rated at their true value), throughout Scotland, with respect to the Pechts, agree in describing those people as decidedly dwarfish in stature. And this belief is most convincingly borne out by the dwellings which the Pechts are believed to have inhabited; the "Pechts' houses" which we glanced at a few paragraphs back, and which speedily led us to consider the Pechts themselves. No man of the average height of modern British people, who has personally inspected these "Pechts' houses", can arrive at any other conclusion than that they were built and inhabited by people of a stature very much less than his own. This is a point so manifest that it need not be emphasised to those who have stooped, squeezed, and crept among the chambers and passages of a "Pictish broch". A few particulars of measurement would quickly convince others; but such details need not be entered into here. However, something may be said with regard to the appearance of the dwelling which may best be regarded as the typical "Pecht's house".

In a *Notice of the Brochs and the so-called Picts' Houses of Orkney*, submitted to the Anthropological Society of London,³ Mr. George Petrie points out that "the name Pict's house is applied indiscriminately, in the northern counties of Scotland, to every sort of ancient structure". And as there is certainly a great difference, in degree, between the various structures referred to, we may here accept Mr. Petrie's guidance as to what constitutes the typical "Pict's house". "The class of buildings to which I have for many years restricted the appellation of *Pict's house* have been", says Mr. Petrie, "very different from the brochs,⁴ both in external ap-

¹ *The Topography of the Basin of the Tay*, by James Knox, Edinburgh, 1831, p. 108. This writer adds that "they are said to have been about three or four feet in height"; and it may be mentioned that when I asked my young guide at Kettlester the exact height of the small Pechts he had just been speaking of, he said, "About that height", indicating at the same time a stature of three feet or so. Whatever their height really was, this young Shetlander's ideas were in agreement with those held "throughout Scotland".

² *Popular Rhymes of Scotland*, 1870, p. 80.

³ See the Society's *Memoirs*, 1865-6, vol. ii, pp. 216-225.

⁴ The term "broch" has been used in a general sense in this paper. This its etymology permits: for it is the same word as *borough*, *burgh*, *burg*, *barrow*,

pearance and general structure and arrangements. The *Pict's house* is generally of a conical form, and externally closely resembles a large bowl-shaped barrow. It consists of a solid mass of masonry, covered with a layer of turf, a foot or more in thickness, and has a central chamber surrounded by several smaller cells. The entrance to the central chamber from the outside is by a long, low, narrow passage; while the cells are connected with the chamber by short passages of similar dimensions to the long one. The walls of the chambers and cells converge towards the top, where they approach so closely that the aperture can be spanned by a stone a couple of feet in length." The most perfect structure of this kind in Orkney is the well-known Maes-how. Maes-how does not appear to be popularly styled a Pict's house, but Mr. Petrie rightly regards it as belonging to that class of building, only "more carefully and elaborately constructed".

Another writer¹ describes a Pict's house—that on Wideford Hill, near Kirkwall—in these terms: "All that meets the eye at first is a green, conical mound, with an indescribable aspect of something *eerie* and weird about it, resting silently amid the moorland solitude. On closer inspection we discover an entrance passage, about eighteen inches high and two feet broad, leading from the lower side into the interior of the prehistoric dwelling,"—and so on.

The resemblance between this kind of dwelling, or its more modern representative, the "bee-hive" hut of the Hebrides and Western Ireland, to the dwellings of modern Eskimos has long been recognised. But it may be permitted to quote here from the accounts given by two Arctic voyagers of the early part of this century, especially as these accounts, both relating to the most northern tribes of Greenland, appear to describe with peculiar exactness the "Pict's house" of Mr. Petrie.

Captain Scoresby, in the account of his explorations in the year 1822, thus describes the deserted dwellings of some of those northern Eskimos:

etc. But the students of these ancient structures have recently restricted "broch" to the more elaborate and superior building of the round or "martello" tower order. This definition is very convenient, and saves much confusion. In spite, however, of the great difference that Mr. Petrie speaks of, as between the so-called "Pictish" broch and the humbler dwelling that alone is recognised by him as a "Pict's house", it is yet evident that the "broch" is to a very great extent evolved from the more primitive and rudimentary "Pict's house".

¹ Mr. Daniel Gorrie, in *Summers and Winters in the Orkneys*, London, 1869, p. 117.

"The roofs of all the huts had either been removed or had fallen in; what remained, consisted of an excavation in the ground at the brow of the bank, about 4 feet in depth, 15 in length, and 6 to 9 in width. The sides of each hut were sustained by a wall of rough stones, and the bottom appeared to be gravel, clay, and moss. The access to these huts, after the manner of the Esquimaux, was a horizontal tunnel perforating the ground, about 15 feet in length, opening at one extremity on the side of the bank, into the external air, and, at the other, communicating with the interior of the hut. This tunnel was so low, that a person must creep on his hands and knees to get into the dwelling: it was roofed with slabs of stone and sods. This kind of hut being deeply sunk in the earth, and being accessible only by a subterranean passage, is generally considered as formed altogether under ground. As, indeed, it rises very little above the surface, and as the roof, when entire, is generally covered with sods, and clothed with moss or grass, it partakes so much of the appearance of the rest of the ground, that it can scarcely be distinguished from it. I was much struck by its admirable adaptation to the nature of the climate and the circumstances of the inhabitants. The uncivilised Esquimaux, using no fire in these habitations, but only lamps, which serve both for light and for warming their victuals, require, in the severities of winter, to economise, with the greatest care, such artificial warmth as they are able to produce in their huts. For this purpose, an under-ground dwelling, defended from the penetration of the frost by a roof of moss and earth, with an additional coating of a bed of snow, and preserved from the entrance of the piercing wind by a long subterranean tunnel, without the possibility of being annoyed by any draught of air, but what is voluntarily admitted—forms one of the best contrivances which, considering the limited resources, and the unenlightened state of these people, could possibly have been adopted."¹

Scoresby's description fully corroborates that given by Captain Ross a few years earlier, when relating his visit to the Eskimos living about the north-eastern corner of Baffin's Bay. These people he describes as "short in stature, seldom exceeding five feet", and he mentions that their sorcerers alleged "that it was in their power to raise a storm or make a calm, and to drive off seals and birds". With regard to their dwellings, he says:

"None of their houses were seen, but they described them as built entirely of stone, the walls being sunk about three feet into the earth, and raised about as much above it. They have no windows, and the entrance is by a long, narrow passage, nearly under ground. Several families live in one house, and each has a lamp made of hollowed stone, hung from the roof, in which they burn the blubber of the seal, etc., using dried moss for

¹ This extract is quoted from the review in the *Scots Magazine* of 1823 (pp. 457-8) of Captain Scoresby's *Journal* (published 1823).

a wick, which is kindled by means of iron and stone. This lamp, which is never extinguished, serves at once for light, warmth, and cooking."¹

It is not out of place to refer here also to an instructive article on "The Archæology of Lighting Appliances", read before the Society of Antiquaries of Scotland by Mr. J. Romilly Allen, F.S.A.Scot., in the course of which he describes the stone lamps found in the habitations known as "brochs" (and popularly assigned to the Picts), with regard to which lamps he states that although not quite identical in shape with those used by modern Eskimos, they are substantially identical, and must have been used in precisely the same way. Comparing this with Baron Nordenskiöld's accounts, Mr. Romilly Allen observes: "The picture here given of the domestic life of the Eskimos at the present time enables us to form a tolerably correct idea of the way in which the inhabitants of the Scottish brochs lighted their dwellings during the long winter nights two thousand years ago." (*Proceedings of Soc. of Antiq. of Scot.*, 1887-88, p. 84.)

From all of these remarks, then, it will be seen that the dwelling of the dwarfish Eskimo and the "house" assigned by Scottish tradition to the Pechts, or dwarfs, are substantially one. And a consideration of the statements also demonstrates clearly that, whatever the age of the word "pecht", none but a race of dwarfish stature would have built such places of abode. Indeed, the stature of the dwellers in the Pecht's house is doubly impressed upon the memory of the Northern Islanders. When Mr. Gorrie describes its outward appearance, he tells us (in similar terms to the Arctic voyagers), that "all that meets the eye at first is a green, conical mound . . . resting silently amid the moorland solitude". But he really repeats himself, although he is not aware of it, when he refers on another page² to "the simple superstition (?) long prevalent among the inhabitants of Orkney and Zetland, that the strange green mounds rising by the sea-side and on solitary moors, were the abodes of supernatural beings known by the name of Trows". Of the "supernatural" attributes assigned to those people, or claimed by them—in early Scotland, in Lapland, and in Greenland—something remains to be said. But the people just referred to under two different, but synonymous, names, are undoubtedly one and the same.

The Pechts of history, then, were a race of dwarfs. Thus, when

¹ From an extract contained in the review (*Scots Magazine*, 1819, vol. iv, pp. 332-3) of Capt. Ross's account (published by John Murray, London, 1819).

² *Op. cit.*, p. 119.

Dr. Skene identifies the Feens of Gaelic folk-lore with the historic Pechts, he reveals them to us as a race of dwarfs. Therefore, the traditional story of the Feen chief's visit to the "country of the big men", where he was regarded by that latter race as a "droich", is entirely in accordance with Dr. Skene's belief that the Feens were of the same race as the historic Pechts. It is not at all unlikely that this identity was taken for granted long before the nineteenth century, and in Scotland. In Allan Ramsay's *Evergreen*, a collection of Scottish poems written before the year 1600, there is a certain "Interlude of the Droichs", also referred to as "The Droichs' Part of a Play". Now, the spokesman of these droichs (or trows, or dwarfs) announces himself as a grandson of Fin, the great chief of the Feens of Ireland. And he makes a statement which is identical with one contained in a Feenic poem on the battle of Gawra. This statement need not be particularised here, but it tells us unmistakably that these "droichs" were regarded as the representatives of Fin and his Feens.¹ Therefore, it would appear from this poem that Fin and his Feens were regarded by the ruling class in Scotland, prior to 1600, as dwarfs. That is, as *pechts*.

Within the limits of this paper it is impossible to deal with even a few of the many points raised by the acceptance of these deductions. But all that has here been said tends to show that the *Feinne* of Gaelic folk-lore, and the Finns of Northern history and tradition, ought to be regarded as one and the same people. And that one section, at any rate, of such people ought to be identified with the Pechts, or Picts, of history.

DAVID MACRITCHIE.

¹ The fact that the "Interlude" is allegorical does not at all affect the question.

DOMESDAY MEASURES OF LAND.

IT had been my wish, in my previous paper (*Arch. Rev.*, June 1888) on the above subject, to spare Mr. Pell as much as possible, and not criticise his eccentric theories so severely as they deserved. Since he has thought fit, however, to retort in angry language, I propose now to expose frankly one of the most mischievous heresies that has ever been advanced.

But let me first point out that, in his attempted reply, Mr. Pell does not even venture to meet my cardinal objection—viz., that while rightly urging “that the terms made use of in reference to the lands on which the taxation was laid must have been of a kind so certain and so sure, that, when any portion of the survey was sent to the king’s officers, it would carry on the face of it the information required, without the need of a local interpreter to explain its meaning”—he yet claims for himself the right to interpret these sure and certain terms in any one of half-a-dozen different meanings that may suit his purpose.

Further, I proved by mathematical demonstration that where, as in the case of Burwell, we can test the truth of his arbitrary interpretation, it is found to be quite erroneous. This objection is passed over by Mr. Pell in significant silence.

Thirdly, I established by mathematical demonstration that Mr. Pell’s assumption as to the *Domesday* text meaning the exact reverse in some counties of what it means in others is “simply blown to pieces” by record evidence. This objection, which of itself vitiates all his calculations, is dismissed by Mr. Pell as “certain weak criticisms”. He does not venture, however, to dispute my evidence, nor can he.

Mr. Pell’s charge, that I have “omitted to state correctly” his words, appears to be based on the sentence (quoted by me)—“under what circumstances, and why this should have been the case, it is hard to say.” These words will be found on p. 69 of his paper on the “Domesday Geldable Hide” (*Camb. Ant. Soc. Trans.*).

I can, however, convict Mr. Pell of completely misrepresenting my own words. He shelters himself triumphantly (p. 358) behind Canon Taylor, who agrees with him, he says, as to certain counties,

and whose calculations, I admit, have attained "marked success". But I had carefully guarded myself, it will be found (vol. i, p. 286), by the words—

"Canon Taylor has attempted the task for the carucates in the East Riding of Yorkshire, and appears, *within that limited area*, to have attained marked success."

With the Canon's hint as to possibilities outside "that limited area" I do not agree, any more than I agree with Mr. Pell, who has, consequently, no right whatever to say that I do.

Having now briefly illustrated the character of Mr. Pell's reply, I proceed to dispose of his theories, and of the arguments by which they are supported.

Let us first take Mr. Pell's own manor of Wilburton, of which his knowledge is more exhaustive, we learn, than of any other.¹ To its details he is continually referring, as to a test-case. Now here, as elsewhere, Mr. Pell claims to establish that the surveys of 1277 and 1221 give us exactly the same area as that which is recorded in *Domesday*, and exactly the same number of acres to the virgate.² This is, indeed, the essence of his case, the proof that his theories are correct. Accordingly, in the case of Wilburton, he first gives us the area recorded in *Domesday Book*, of which the alleged aggregate is 864 acres, and then gives the area recorded in 1277, of which the alleged aggregate is 864 acres, "making the exact quantity that was found to be in *Domesday* and the *Inquisitio Eliensis*."³ And in each case the area of the virgate is precisely the same, namely, 24 acres.⁴ This convincing proof rests on two legs: (1) That *Domesday* records an aggregate area of 864 acres (with a virgate of 24 acres); that the Survey of 1277 records an aggregate area of 864 acres (with a virgate of 24 acres). With the second of these demonstrations I am not now concerned. It may be right, or it may not. In any case, I confine myself to *Domesday*. How, then, does Mr. Pell extract from *Domesday* the above figures? By the simplest of all processes. He finds that what he requires to prove for his purpose is, that the

¹ "It is only by obtaining more detailed information that a correct judgment can be formed; and fortunately, in some cases at least, we have it. I propose to take the manor of Wilburton as a first example, because I have the means of testing, and have tested, its case," etc. (*C. A. S. C.*, vol. vi, p. 21.)

² "He further stated that he had found no instance in which *Domesday Book* and the two MSS. did not agree as to the area of the virgate of the homines in the manor so surveyed." (*Ibid.*, p. x.)

³ *Ibid.*, p. 26.

⁴ *Ibid.*, p. 100 (Table I).

terra ad unam carucam of the *homines*, in *Domesday*, was here 108 acres, composed of four-and-a-half virgates of 24 statute acres each. And how does he prove it? *He simply assumes it!* Here are his own words:—

“Assuming for the present that at Wilburton the *terra ad unam carucam* of the *homines*, at any rate, consisted of four-and-a-half virgates of 24 statute acres each, and each being a *plena terra*.”

These words, which occur incidentally between brackets, are all the proof Mr. Pell attempts to adduce. *Domesday* tells us nothing more than that there were seven plough-lands,¹ of which three were *in dominio*; but Mr. Pell unhesitatingly assumes precisely what he has to prove, namely, that the four remaining plough-lands consisted each of 108 acres, divided into four-and-a-half virgates. Now, if we turn to his Table III (p. 100), we find that for the dozen manors, of which Wilburton is one, he allows himself a range of from 60 acres to 120 acres for the area of the tenant's *terra*, and of from $1\frac{1}{2}$ to $4\frac{1}{2}$ virgates as its contents. What ground, then, has he for assuming that in this case of Wilburton, and in this case alone, the area was 108 acres, and the component virgates $4\frac{1}{2}$? *None whatever*, either from *Domesday* or from the *Inquisitio Eliensis*. He has simply pitched on these figures because they are those which he requires, and, having done so, he actually produces them as proof that his theories are correct. Surely, no more startling inversion of proof has ever been gravely set forth by mortal man!

But even this is not all. Mr. Pell, not being able, even by this assumption, to evolve from *Domesday* that area of 864 acres which he requires, boldly inserts “*Pratum*, 39 acres”, and thus produces his total. Now, remember that this total is professedly extracted from the evidence of “*Domesday*” and the “*Inquisitio Eliensis*”. Yet, in neither of these records is there one word about *Pratum*, still less about 39 acres! They are absolutely silent on the point.

Now I am sorry to say that, after careful study and thorough dissection of Mr. Pell's tables, I am satisfied that we have here the key to all his elaborate calculations. Starting, in the case of each manor, from the virgate of a thirteenth-century record, he has simply assumed that the virgate of *Domesday* contained the same area, and has then modified (I am sorely tempted to use the word “cooked”) the language of *Domesday* to suit that assumption. Having done so, he confidently appeals to the fact that in every instance *Domesday Book* and the Surveys of the thirteenth cen-

¹ “*Terra est septem carucarum.*”

tury "agree as to the area of the virgate of the *homines* in the manors so surveyed"!

Anyone can test my conclusion for himself. Let him take as his starting-point the parallel columns in which Mr. Pell has placed side by side the area of the virgate in *Domesday Book* and in the thirteenth-century MS. Let him then trace how the language of *Domesday* is modified (tampered with, the student may be tempted to complain) to arrive at the required area. Infinite are the modifiers in power. For instance, x hides *plus* 1 virgate may mean x hides *plus* 1 virgate, or x (hide + 1 virgate), or simply x hides ("*Anglico numero*"), as you please. The geldable hide itself connotes an area varying from 120 acres to 288, and may contain any number of virgates from 3 to 20.¹ The *terra* of *Domesday* may mean anything from 30 acres to 120, and may contain any number of virgates from 1 to 9. Then you can also count 5 as 6, if convenient (not systematically, but wherever you like). Lastly, if, even with these resources, you cannot bring out the result you want, you have only to place "extra hidam" any number of inconvenient acres. Mr. Donnelly's handling of Shakespeare's text is hopelessly eclipsed by such a method as this. Before Mr. Pell's interpretation of *Domesday*, the mathematics of the Great Pyramid pale their ineffectual fires.

Thus is solved the problem which has baffled so many students. In vain have they pored over *Domesday Book*, struggling to discover by what means Mr. Pell evolves from its text information which it does not contain, and wondering how he knows in every case which of his many interpretations to select. If they take my advice, and start from, instead of working to, the virgate required, the whole method will be clear at once, and they will find that Mr. Pell's interpretations are based on arbitrary assumption alone. There is nothing in *Domesday* to support them. Surely, the student has a right to complain when the evidence of *Domesday* and the assumptions of its interpreter are so inextricably intertwined that every assertion has to be tested before we can accept it as fact.²

¹ See *C. A. S. C.*, pp. 100-101 ("Table I : Number of virgates in one hide").

² Take, for instance, the contents of Wilburton Manor, *temp. Domesday*, as set forth by Mr. Pell on the authority of *Domesday Book* and (the fuller version of the original returns contained in) the *Inquisitio Eliensis* (*C. A. S. C.*, vi, p. 22). There we read: "*Terra ad carucam* in the occupation of the *homines in opere*, 300 acres"; yet no "*homines in opere*" are mentioned in either record, nor anything corresponding to "300 acres". And when Mr. Pell tells us that "the whole *terra ad carucam* of the *homines*" was "equal to

The crowning evidence, however, is coming, and may be left to speak for itself. In this, his own manor of Wilburton, Mr. Pell tells us that the "tres hida et una virgata" of *Domesday Book* and the *Inq. El.* means "three hides *plus* one virgate of 24 acres" (p. 21), *i.e.*, "384 acres". Good! Now turn to p. 92, and we find him referring to this passage for full details, and then assuring us that "the lord's III hidæ et I virg. is really 3 (120 + 24), not 360 + 24. There are other cases like this in *Domesday Book*". That is to say, that, without even being aware that he is flatly contradicting himself, he first tells us that "tres hidæ et una virgata" means 360 + 24, and then that it does *not* mean 360 + 24, but 3 (120 + 24), *i.e.*, 360 + 72! See the result of tampering with the text of *Domesday Book*. You first claim the right to interpret a formula in either of two opposite ways, as may be most convenient to yourself, and you end by interpreting it, all unwittingly, in both!

Here is another example of Mr. Pell's unfortunate habit of jumping recklessly at erroneous conclusions. He appends to his Huntingdonshire tables this note:

"There are repeated entries in *Domesday Book* of the following kind: In dominio sunt (x) car. 'præterea predictas hidas', 'exceptis his hidis', 'extra hidas'. The meaning of such entries is disclosed by the *R[amsey] C[artulary]*, for instance, at Slepe (*post*, No. 87). *R. C.*, at p. 282, speaking of the *dominium*, states, 'Coli possint sufficienter cum tribus carucis propriis et consuetudine carucarum villæ et duabus precariis carucis quæ consuetudo ad valentiam trium carucarum estimatur.' The meaning being evidently that, over and above the help derived from the tenants' hides or ploughs, the lord had three ploughs of his own."¹

That is to say, that (to take an instance from Mr. Pell's own table), "VII hid' ad geldum. Terra x car'. Et in dominio terra II car' extra predictas hidas,"² means "evidently" that there were two ploughs in demesne "over and above [the help derived from] the tenants' hides [or ploughs]."

To this I reply:

(1) That there are *no such entries* in *Domesday* as Mr. Pell describes. They all say that there are so many plough *lands* (not *ploughs*), "præterea predictas hidas," etc.³

the work of four eight ox-ploughs, as stated in *Domesday Book*, taking 108 acres each", we have to distinguish between the "four ploughs" which *do* occur in *Domesday*, and the 108 acres which are Mr. Pell's alone.

¹ *C. A. S. C.*, No. xxvii, p. 103.

² Witune, *ibid.*, p. 143.

³ "Terra II car[ucis]," *ut supra*,

(2) That these plough-lands were "over and above" the "*hides*" (not *ploughs*), which is very different.¹

(3) That the meaning of these entries is "disclosed" in express words by *Domesday Book* itself, and is, of course, quite different from that which Mr. Pell gives.

The explanation given in *Domesday Book* is simplicity itself. We learn that, by a special arrangement, throughout the hundred of "Herstingestan", the demesnes paid no geld. All the assessed geld was paid by the hidated *Villenagium*.² This peculiar arrangement explains the peculiar formula so completely misunderstood by Mr. Pell. Throughout the hundred we find the demesne (non-hidated) sharply distinguished from the (hidated) *Villenagium*, and surveyed after it. I will here give the true rendering of the entries selected by Mr. Pell for his table³:

Name.	Plough-lands.	Villenagium (hidated).		Dominium (non-hidated).	
		Ploughs.	Hides.	Plough-lands.	Ploughs.
Bluntisham - -	8	3	6½	2	2
Hoctune - -	10	10	7	2	2
Witune. - -	10	8	7	2	2
Riptune - -	16	12	10	2	2
Stivecle - -	11	9	7	2	2
Slepe - -	24	26½	20	3	3
Upehude - -	16	14	10	3	2
Colne - -	6	5	6	2	2
Haliewelle - -	9	7	9	2	2
Wistow - -	16	11	9	3	2
Wardebusc - -	20	16	10	3	3

A glance at this table will show how appropriate is the language of *Domesday* in its surveys of these manors. Comparison with *Domesday* will show that my figures are taken straight from it without any charge of tampering. But, comparing it with the table given by Mr. Pell,⁴ the two will be found hopelessly different, Mr. Pell having mangled the figures of *Domesday* beyond all recognition. His cardinal mistake in including the ungeldable and non-hidated *dominium* in the hide-assessment would alone

¹ Mr. Pell, *more suo*, calmly interpolates the words I have placed in square brackets, rendering "*hidas*" as "*hides or ploughs*"!

² "In Herstingest' Hundred sunt dominicæ car' quietæ de geldo regis. Villani et soch'i geldant secundum hidas in brevi scriptas" (I, 203).

³ *C. A. S. C.*, vi, pp. 143-4.

⁴ *C. A. S. C.*, vi, pp. 105-6.

deprive his calculations of all value.¹ There was, fortunately, one exception to the rule in this hundred, which gives us an independent test. *Domesday*, having recorded the rule (*ut supra*), proceeds: "Excepta Broctone, ubi geldat abbas cum aliis pro una hida" (204). Now Broughton is selected by Mr. Pell, unluckily for him, as a case for special investigation. He discusses it at considerable length, not only in his tables, but in the text.² He tells us confidently, in this case, that—

"The lord pays on the lord's land (4 hides), as we learn from *Domesday Book* that he had 4 car'. This leaves tenants' land, on which the gheld of five more hides was to be paid."

Now the passage quoted above from *Domesday* distinctly states that the lord paid not on *four* hides, but on *one* ("pro una hida"). The whole of Mr. Pell's elaborate calculations being based on his unsupported (and wholly erroneous, as it proves) assertion that the lord paid on *four* hides, they come here, as elsewhere, toppling to the ground.³

Another instructive instance must be set on record. When we are told, with such absolute confidence, that "the Norman king's officers" have done certain things, "have reduced" the area at "Chillelesla" or "reduced the [wholly imaginary and erroneous] terra of the car. of 108" at Wilburton; that in one place they "take off a third for fallow", in another "take off one-half for fallow", or that, in another, "off every virgate the king's officers take and place 'extra hidam' a sixth, *i.e.*, 4 acres, making the virgate 20 acres, of which there would be, of course, six in the geldable hide of 120"⁴—we must, I fear, for "the king's officers", read "Mr. Pell". For never a word has *Domesday* to say concerning these wonderful operations. Elsewhere, however, Mr. Pell detects the evidence that *Domesday* fails to afford:

"In some cases in MSS. the definite names of 'sexacra' and 'sexlond' was given to the sixth part. Thus, in the *Domesday* of St. Paul (Hale, p. 46) there will be found an entry," etc.⁵

¹ The odd thing is that Mr. Pell's own record, the *Ramsey Cartulary*, as quoted by him for "Wardebusc", distinctly confirms *Domesday*, saying: "In villa de Wardeboys sunt decem hydæ terræ præter dominium Abbatis."

² *C. A. S. C.*, vi, pp. 92, 103, *ante*, vol. i.

³ The importance of such a case lies in the difficulty of applying any test to Mr. Pell's fanciful calculations. Here we can apply an independent test, and with significant result.

⁴ *Domesday Studies*, i, 353-7.

⁵ *C. A. S. C.*, p. 94.

Now at first sight, no doubt, on referring to this passage, such entries as "II sexacras pro IIII d. extra hidam", "I acram saxacram (*sic*) pro II d. extra hidam", appear to be quite in favour of Mr. Pell's theory. But what are the facts? Mr. Pell claims that "*a sixth part*" was thus placed "extra hidam"; yet in this, the only instance he can produce, the "sixth part" should be 192 acres, whereas the actual acres "extra hidam" are here only 6! In other words, we have in this passage the details of 8 hides, each with a recorded area of 120 acres; each, therefore, according to Mr. Pell, should have 24 acres ($\frac{1}{6}$ of 144) "extra hidam". Instead of which, one of them has 4 of such acres, another has 2, and the remaining six have none at all! Such is the evidence Mr. Pell produces, evidence which, he must himself have seen, in no way supports his case.

Nor is even this all. Hale, the editor of the record, corrects, in a note on this passage, the printed "sexacras", and writes—

"By an error of transcription 'sexacras' has been printed instead of 'seracras', and subsequently 'sexlond' instead of 'serlond'" (p. lxxvii).

The ground is thus cut from under Mr. Pell's feet. But does this dismay him? Not at all. He calmly informs us that

"Archdeacon Hale has yielded to the mistaken suggestion of a friend, and has confessed to an error of transcription where there has been no error at all."¹

Now, in the first place, it is absolutely contrary to fact that Archdeacon Hale, in this reading, "yielded to the mistaken suggestion of a friend". The correction is his, and his alone.² In the second, what possible ground can Mr. Pell have for coolly asserting that an editor "has confessed to an error of transcription where there has been no error at all"? Does he profess to have even seen the MS.? He does not. He waives aside the editor's correction of his own reading on no other ground than that which is always sufficient for him—that it would be fatal to Mr. Pell's argument!

Here, then, we have a type of these arguments, to test which students are compelled to waste their time and labour. Mr. Pell's reference to "some cases in MSS." is reduced, on investigation, to a single MS., and to a single manor in that MS. Even in that solitary case the evidence is seen at once to be against, not for,

¹ *Domesday Studies*, i, 359.

² Being in doubt as to what "*seracres*" might mean, Hale inserts, in a foot-note, an "illustrative conjecture" by a friend *as to the meaning of "ser"*. The "friend" was not consulted as to *the reading*.

Mr. Pell ; while, lastly, the evidence itself is found to consist of a misprint, which Mr. Pell desperately, but in vain, endeavours to ignore.

The burden of my complaint is this. Mr. Pell leads us at every step to believe that he has absolute proof for his assertions when that proof consists of nothing but a solitary misprint or clerical error, which he tortures into evidence for his theory, or worse still, of sheer assumption. Really, this is not fair to other students of the subject.

As examples of misprints and clerical errors, let us take his assertion that his extraordinary thesis as to *Domesday* requiring, in certain counties, to be read topsy-turvey,¹ "is proved in another way, namely, by the discovery of cases in *Domesday Book* where the word *hida* is in the same *Survey*² used in more than one sense".³ As "a case in point", we are referred to the Cornish manor of "Pavtone". Here I give the relevant passage from *Exon Domesday*, with the manor which immediately succeeds it in Mr. Pell's own list (Table III) as a parallel :—

"PAVTONE.

"In ea sunt XLIII hidæ . . . Has possunt arare insimul LX carr. De his habet Episcopus in dominio 1 hidam et III carr.; et villani habent XLIII hidas* et XL carucas."

"LISCARRET.

"Ibi sunt XII hidæ. Has possunt arare LX carr. De his habet comes, in dominio 1 hidam et III car.; et villani XI hidas et XIII car."

The system on which the entries are drawn up is self-evident. We have 44 hides = (1 + 43), and 12 hides = (1 + 11). But the copying scribe of the *Exon Domesday*, by the most natural of clerical errors, wrote "car[ucas]" instead of "hid[as]", where I have placed an asterisk.⁴ As this made nonsense of the passage, he saw and corrected his mistake, and substituted the required "hidas". Mr. Pell, however, seizes on the slip as proof, "if not indicating an erasure" (which is precisely what it does), "that 'hid' and 'carr.' meant the same thing." And on this assumption he proceeds to build the most elaborate calculations. So, too, in his Surrey case ("Estreham"). Here he quotes *Domesday* as reading "tunc se defendit pro v hidis et modo similiter pro i hida et i virg' terra". Plausible as his explanation appears, it is based on a misquotation. What *Domesday* really reads is, "Tunc se defendebat pro

¹ See *ante*.

² The italics are Mr. Pell's own. ³ *C. A. S. C.*, p. 77 ; *D. S.*, i, 325.

⁴ The same mistake is made on folio 205b of *Domesday Book*, where it is corrected in precisely the same manner.

v hidis et modo similiter." Over the word "similiter" an interlineator has inserted the words, "Pro una hida et I virgata terre." Mr. Pell has jumped at the conclusion that these words should be read as an addition, and as this would make nonsense of the passage, he boldly assumes that the first hide was not a hide, but a virgate.¹ Unfortunately for him, the manor which, in *Domesday Book* (34b), immediately precedes "Estreham" (but to which he makes no allusion), has a similar interlineation, which is completely destructive of his view, for it gives us "I hida" over "similiter", as the assessment of an eleven-hide manor. It is thus established that these interlineations are not (as Mr. Pell wrongly assumes) additions to, but corrections of, the word "similiter".

In the third case² ("Chenebalton", in Huntingdonshire) we can establish with equal certainty that Mr. Pell's interpretation is wrong. In this district the formula for expressing the hidation of the demesne was this: "Ibi habet in dominio x car. in y hidis hujus terræ." There are seven cases of this on the "Chenebalton" folio (205b) alone. In the "Chenebalton" entry the scribe had omitted the "in y hidis", but, detecting his omission, had corrected it by interlineation. Mr. Pell here bases his calculations on an assessment of 10 hides for the tenants,³ whereas their assessment, we learn from *Domesday*, was only 4 hides. Here, then, again, his explanation is simply inept.

I might thus dispose of case after case, and prove it to rest on nothing but assumption, assumption which, when it can be tested, is erroneous to demonstration. But I will content myself for the present with exposing a fundamental assumption which runs through the whole theory. Speaking of "the *Domesday* geldable hida or carucata", Mr. Pell writes:

"The *Domesday* geldable hide of 120 acres was the kernel of Fleta's carucate, which seems to have been composed of the sown land, linked with its twin brother the land *ad warectandum*, and if lying in common when fallow, then *extra hidam*. This sum total of land *ad geldum*, and land *extra hidam*, appears to have been in two shift manors, 240 or 288 acres, and in three-course manors, 180 or 216 acres."⁴

Now, whatever Fleta's carucate "appears to have been" to

¹ There is a case in Surrey which shows that an area which had been expressed T. R. E. in hides (meaning thereby virgates) is converted in *Domesday Book* into the uniform hide of 120 for taxational purposes (*C. A. S. C.*, p. 78).

² *C. A. S. C.*, p. 78.

³ *C. A. S. C.*, p. 103.

⁴ *Domesday Studies*, p. 349.

Mr. Pell, it *was*, as he elsewhere admits, 160 acres¹ in a two-course manor, *not* 240.²

But as his "kernel" of 120 acres cannot be adapted to an area of 160, he boldly invents (*ut supra*) an area of 240 (or 288) acres (for two-course manors), absolutely unknown to Fleta! Such is the origin of this mysterious area, Mr. Pell's authority for which it has baffled students to discover. Its inventor writes, that "our forefathers were more advanced than the modern crab-fishers of Cromer" when they evolved this "hide of wara" of 288 acres.³ Mr. Pell is really too modest. He should not give "our forefathers" credit for an invention of which he is himself the sole patentee.

¹ *Domesday Studies*, pp. 187, 188, 323.

² I omit the *Anglico numero* reckoning in each case.

³ *Domesday Studies*, p. 359.

J. H. ROUND.

NEW VIEWS ON THE KALEVALA.¹

THIS youngest addition to the ranks of folk-lore periodicals is as yet but little known in England. It deserves, however, a wide circulation, if only for the fact that it chronicles the movement of folk-lore studies in lands extremely rich in tradition, and the languages of which are unintelligible to the vast majority of English students. The editor is one of the chief authorities on the mythic and traditional lore of the Wends and Lithuanians.² It is therefore not surprising to find that the folk-lore of these races, as well as of the Slavs generally, and of the Finns, receives the chief share of attention in the *Zeitschrift*. Folk-tales, local legends, notes of superstitions, reviews of books and periodicals, form the staple of this as of other similar journals, and it would, in an ordinary way, be sufficient to point out the importance of the *Zeitschrift* as the interpreter of Eastern and North-Eastern European folk-lore. Three articles, however, at least, deserve fuller notice, dealing as they do with problems to the elucidation of which the ARCHÆOLOGICAL REVIEW is specially devoted. Two of these are by the Editor—on the Rubenzahl legend, familiar to students of German literature through the graceful version of Musäus, and on the Aryan fire-myth as set forth in the Wieland saga. As a mythologist Dr. Veckenstedt occupies a position of his own, with decided leanings towards our anthropological school, with a decided distaste for the systematic extravagances of the “nature-myth” theorists, but with almost equal distrust of the large and loose generalisations of professed anthropologists, and with candid recognition of the importance of nature-myths in the religion of low-cultured races. He always endeavours to realise the totality of the social and mental conditions of a race, and to judge whether the interpretation of a myth accords with them or not. Thus, he holds that mankind

¹ *Zeitschrift für Volkskunde*. Herausgegeben von Dr. Ed. Veckenstedt. Nos. 1-9. Leipzig: A. Dörfflel.

² (1) *Die Mythen, Sagen und Legenden der Zamaiten*. Heidelberg, 1883. (2) *Pumput ein Kulturdämon der Deutschen, Wenden, Litauer und Zamaiten*. Leipzig, 1885. (3) *Sztukoris, der Till Eulenspiegel der Litauer und Zamaiten und Schut Fomka sein russisches Ebenbild*. Leipzig, 1884. (4) *Wendische Sagen, Märchen und abergläubische Gebräuche*. Graz, 1880.

must have obtained fire in the first instance from the striking of a flint, rather than from the friction of wood, and he is thus led to arrange the fire-myths in a different order of precedence, and to interpret them otherwise than Adalbert Kuhn in his well-known *Herabkunft des Feuers*. This article is not as yet finished—a discussion of the most celebrated of fire-myths, that of Prometheus, will appear in No. 10—and it is difficult to judge how far the case is made out. The most novel portion to the English reader will be the Lithuanian fire-myths; these are given at length, and can hardly fail to arouse a desire to know more of this rich and picturesque mythology.

Dr. Veckenstedt's exposition of the Rubezahl story affords less room for comment or dissent. These legends have come down to us in a debased and often artificial condition, and their correct interpretation must have been a matter of no small difficulty. Applying his knowledge of Slavonic folk-belief, Dr. Veckenstedt makes it almost certain that the tricky and malicious lord of the Riesengebirge was originally a water-sprite. The numerous analogous stories quoted from Wendish and Lithuanian sources are of great interest.

The article, however, which deserves the most attention is one by the late distinguished Finnish scholar, Dr. K. Krohn, upon the composition and date of the Kalevala. It is, unfortunately, only the bare summary of a long argument based upon a multiplicity of detail-considerations which are not given. It would be evident from the article, if it were not matter of common repute, that the author has studied the Finnish folk-epic most exhaustively, and that his conclusions are entitled to the utmost respect. It is also evident that Dr. Krohn adopted, with regard to the nature and mode of diffusion and transmission of popular tradition, views to which there are strong objections on *a priori* grounds, and the evidence against which is, to many minds, overwhelming. Dr. Krohn found, in what may be called the borrowing-theory, support for his conclusions, and it is not surprising that in return he urges his conclusions in support of the borrowing-theory. According to him, the Kalevala does not embody the primitive mythic beliefs of the Finnish race, nor can it be used, save to a very slight extent, in any attempt to illustrate an early phase in the mythology of mankind. It consists to a very large extent of episodes which had originally no connection, whether epic or mythic, one with the other; these episodes are mainly borrowed from the god- and hero-sagas of the Scandinavians; the combination of episodes and groups of episodes into

a rounded and coherent narrative is of comparatively recent date, and is the work of families of folk-singers chiefly resident in the Archangel government, among a population which had in reality lost touch with the older and ruder separate lays, so that the reciters were able to give freer rein to their artistic spirit. Lönnrot may be regarded as the last and greatest of these folk-singers; his unrivalled knowledge of Finnish folk-song enabled him to fill in transitional matter between the episode, and to weld the whole into one continuous narrative.

Dr. Krohn's conclusions may, it is seen, be classified under two heads—those relating to the manner in which the Kalevala has grown to its present shape, and those relating to its subject-matter. Assent to the one set of conclusions does not necessarily involve assent to the other. The first point could only be determined by an examination, as careful as Dr. Krohn's, of the immense mass of folk-variants of particular episodes, out of which the poem in its present shape has been picked and pieced together. Dr. Krohn notes that living reciters are still adapting the traditional lays to the tone of the present day—thus one reciter makes the dread goddess of the otherworld indulge in coffee. This is a feature of tradition which is familiar to all folk-lorists, especially when the material has been collected from a class which has been largely influenced by modern civilisation. To Dr. Krohn's sketch of the hypothetical growth of the poem it is not, however, easy to find a parallel. The closest is furnished by the Arthurian romance. We know, almost certainly, that it is the artificial welding together of a number of episodic lays originally disconnected with each other. But we also know that the authors of this process were the literary class of the day, and we cannot fail to recognise in the finished product all those signs which distinguish the outcome of conscious, freely working craftsmanship from that of tradition. Are we to look upon the professional reciters from whom Lönnrot took down the materials of the Finnish epic as so many Chrestiens de Troies?

Celtic literature affords another parallel in the Fenian saga, but here the analogy makes against Dr. Krohn's theory. Such epic traces as the saga possesses are obviously the work of the Irish literary class at a particular period in the development of Irish letters, and have been entirely discarded by living tradition, which preserves the saga as a series of detached and often discordant episodes with the utmost tenacity. Nor is Macpherson's Ossian a parallel to the Kalevala of Dr. Krohn's theory—the one being a work of individual literary effort raised upon a scanty foundation

of genuine tradition, the other *ex hypothesi* a mosaic of actual tradition, in which the individual literary impulse is limited to the work of arranging and rounding off.

We might, as already said, accept Dr. Krohn's theoretical development of the traditions contained in our Kalevala, and yet join issue with him as to their authenticity. One instance will show how revolutionary is his criticism. The quest for the *Sampo* has always been looked upon as one of the most interesting and curious features of the poem. But, if we believe Dr. Krohn, there is no such thing as the *Sampo*. The story as we now have it is a fusion, probably a very late one, of a cosmogonic myth which may be old and pan-Ugrian, and of an episode imitated from the Scandinavian quern-myth. We can only criticise this theory on general grounds, as Dr. Krohn withholds the specific evidence upon which he bases it. But why, may it be asked, should the Finns be denied that faculty of myth-creation and myth-embodiment which we find possessed by well-nigh every human race? If they did possess that faculty, where are their myths? and why should it be assumed that such as are found among them are borrowed from their Scandinavian neighbours, simply because they are in part similar to those of the latter? Odin wins a magic drink in eagle-guise; Wainamoinen performs a somewhat similar feat; the one incident must be borrowed from the other, says Dr. Krohn. But, as all readers of Mr. Lang know, Yehl, the Thlinkeet deity, is in a like case with the Norse god and the Finnish half-god. Has there been borrowing here too? Did some companion of Leiv or Thorfinn find his way from Vineland¹ across the prairies and the Rockies and charm the Pacific coasters with tales of how Gaut's son beguiled Suttung's maid and won Suttung's mead? Prophecy is proverbially unsafe, or I should feel inclined to prophesy for Dr. Krohn's attack on the authenticity of Finnish tradition a failure as complete and memorable as that of Professor Bugge's onslaught upon Scandinavian mythology.

Dr. Krohn's pronounced adherence to the borrowing theory induces me to say a few words on the subject in reference to my article (*Arch. Rev.*, April 1889), and to that of Mr. Stuart-Glennie (*Arch. Rev.*, May 1889). We are told how the Finns got this or that idea from the Scandinavians, the latter from the

¹ Readers of the *ARCHÆOLOGICAL REVIEW* may be glad to be referred to the recent monograph of Professor Gust. Storm, *Studies on the Vineland Voyages* (Copenhagen, Thiele, 1889), in which the Vineland of the Norseman is shown to be Nova Scotia.

Greeks, they from the Egyptians, and *they*—whence? From the *Arkhaian white race*, Mr. Stuart-Glennie would perhaps say. But did the latter *invent* the idea? Upon what does the tortoise stand?—I coined for this theory the epithet “revelationist”; the epithet has been cavilled at, but nothing has been suggested in its place. For, be it noted, the theory implies that somewhere, to some one particular gifted race, the mass of ideas which underlie and are the raw material of myth, was *revealed*. Otherwise, why the theory? If the *Arkhaian white*, or any other race could, by the exercise of its mental and emotional faculties, originate and develop a system of myth, why should this capacity be restricted to it alone? Bryant and Faber, and Mr. Casaubon, had at least warrant and a groundwork for their building—they postulated a divine revelation. But Mr. Stuart-Glennie and Herr Gruppe—what are they doing in this galley?

It may be worth while to briefly restate the opposing theory. Every human race has at one time passed, or is at present passing, through a definite stage of culture, the distinguishing features of which, on the mental side, are best described by the term animistic. It cannot be asserted that definite social conditions necessarily accompany this mental state, but, as a matter of fact, the two great institutions of matriarchalism and totemism are frequently found in combination with it. Hence, while the mythopoeic faculty, dependent as it is upon the animistic mental state, develops itself in the same way among all races which have passed through that stage of culture, the forms in which that faculty embodies itself, forms necessarily derived from the social condition of the race, also show great sameness, owing to the widespread prevalence of certain customs. A mythic protoplasm is thus obtained, common to all humanity, but worked up in its own way by each race, and the final outcome depends very much upon the stage of culture reached by the race when its mythology becomes fixed. That the mythic systems which thus come into being act and react upon each other, that less developed or gifted races borrow from those more highly gifted or developed—this is not denied, indeed, it is undeniable—and there will always be work for the historian in tracing the growth and determining the extent of such influences. It is, however, contended that the underlying similarity of all mythologies is not due to their being more or less changed copies of one original exemplar, but to their having originated in one special stratum of the mental and social development of mankind. Dr. Krohn's careful analysis has seemingly disproved this theory

in the case of the Kalevala ; a yet more careful analysis may, nay, I believe will, have the contrary result. In the meantime, however, it will be well for those who are not familiar with Finnish, and hence cannot examine the evidence for themselves at first-hand, to refrain from using the Kalevala as if its age and authenticity were unquestioned.

ALFRED NUTT.

NOTES FROM PARLIAMENTARY PAPERS.

No. 9.

CORRESPONDENCE RESPECTING NEW GUINEA. MAY 1883.

THEY relate a curious circumstance in connection with the people of Kerepo. The male children have a hole pierced through the left hand in infancy, which is kept distended like those in the lobes of the ear until they arrive at manhood, and is then put to this extraordinary use: In fighting, their arrows are balanced through the hole in the left hand, from whence they are propelled with great force by a smart blow from the right hand. The bow is not used by them. We classed this story with the account of the men with tails, but the chiefs were quite amused at our incredulity, and assured us they themselves had seen these things.

Just before sundown we were sent for to see a procession of young men and maidens perambulate the village. About sixty took part in the ceremony, the men dressed in all the bravery of paradise feathers and necklets of pearl shell, the women wearing heavy necklaces of dogs' teeth, with pendants of the same reaching to their waists. The women walked backwards, beating small drums with a slow, monotonous chant, while the young men kept time with their feet and long white wands covered with streamers and hollow seeds. The party halted before certain houses and sang, and after reaching the end of the village broke up and dispersed. The greatest gravity characterised the proceedings.

The Boera people are friendly with the Roro tribe on the mainland, opposite Yule Island, who murdered Dr. James and Thorgren in 1876. They gave us a minute circumstantial account of the affair, which agrees in every particular with the evidence given me by the survivors on that occasion. We asked what led to the attack, and they told us the Chief wished to test whether all white men were invulnerable, as a short time before a white man had allowed them to hurl spears at him, which rebounded without doing him any injury. "Was he a spirit? Had he come from the clouds?" they asked. But no, he came from the sea, and their weapons were powerless against him. We set this down as some ancient tradition, but Captain Dudfield afterwards told us he had

seen a coat of mail in Signor D'Albertis' possession, and as that gentleman visited the Roro tribe, there may, after all, be some foundation for their story.

Thanks to Mr. Lawes, we are better acquainted with the legends of the Port Moresby district than those of any other portion of New Guinea, east of the 141st meridian. According to these, they are the conquering race, who are gradually displacing the cannibals on the coast. The Kerepunites, again, are a different race, and are said to resemble the people of Eastern Polynesia. Mr. Chalmers kindly translated for me the following legend, as told in Rarotongan by Peri's wife, who had heard it from the people of Bocra. Ages ago Port Moresby produced fruit in abundance, but a quarrel arose, which resulted in the secession of one-half of the tribe, and from that time the sago-palm and cocoa-nut tree gradually died out. About this time fire, which was previously unknown, was brought to them by a dog. The people had noticed a brilliant light at night and thick smoke by day in a large canoe becalmed off their coast, and knew not what it was. In those days they lived on intimate terms with the birds and animals, and could converse with them. The turtle first volunteered to fetch news concerning this strange light, but failed in the attempt. The pigeon next tried, but fell exhausted into the sea while returning with a flaming brand. The dog then offered his services. He boarded the canoe, and saw water boiled and food cooked by this mysterious light. Watching his opportunity, he jumped overboard with a brand in his mouth, and swam ashore in safety with his prize. He taught the people how to use the fire and to cook their food; but they forgot, in time, the benefits conferred by the dog, and treated him with ingratitude, so he revenged himself by biting the people, and fomented the quarrel which led to the dispersion of the tribe. Those who remained at Port Moresby were reduced to the verge of starvation, but eventually their brethren, taking compassion upon them, taught them the art of making pottery, and sold sago to them in exchange for earthenware pots.

At daylight on July 30 I walked to Kerepunu, and went on board the *Sappho*, and Captain Digby and his officers being anxious to witness the festivities at Kalo, I returned with them in the steam cutter. It being low water, the sea was breaking heavily on the bar, so that it required considerable nerve to cross, especially as we did not know the depth of water, and a monster shark was cruising at the back of the first roller, as if on the look-out for a "fresh mess". Our landing created little excitement, as the people

seemed to have given themselves up to enjoyment, which not even this unheard-of influx of foreigners could disturb. While I was away ten pigs, each weighing about 3 cwt., were sacrificed with all due ceremony at the foot of the food scaffold, and a number of young women, in a state of absolute nudity, threw food to the people from a platform. It appears that at each annual feast the girls who have been tattooed since the last festival are required to perform this duty as evidence of the fact, and I am told that several seemed overcome with shame at the exposure. Mr. Chalmers tells me these people have a distinct idea and beautiful conception of a Supreme Being. To them the Great Spirit is a most beneficent being from whom naught but good is received, while the spirits of their ancestors are blamed for all evil that befalls them. A good yam season is the work of the Great Unknown—a drought is caused by malevolent ancestors. The whole proceedings at this festival seemed to have a religious significance, and it may be regarded as a thanksgiving for a bountiful harvest. The distribution of food went on all day to the people of three villages besides Kalo, namely, Papaka (inland from Hulu), Kamari, and Kerepunu. We estimated that there could not have been less than 30,000 cocoa-nuts and 500 bunches of bananas, besides yams and sugar-cane, on the scaffold poles. Dancing commenced about dusk, but as there were no fires and lights, and women principally took part in it, 'twas a very tame affair; accordingly we started a dance on our own account, and very soon had by far the largest share of spectators, one of whom, a very pretty lassie, deftly abstracted a red silk pocket-handkerchief from my belt.

No. 10.

CORRESPONDENCE REGARDING AFFAIRS OF THE
GOLD COAST.

AGRAVIE is the town which, situate on the right bank of the Volta, marks the southern limit of the Volta district; here there is a mixed population of Awoonahs and Addahs, probably numbering from 2,000 to 3,000. The head chief and Fetish priest is named Gathrakpee; he has reigned as such since 1874.

The election of these chiefs is said to be by the selection of the Fetish. His jurisdiction extends to Sopey and Tefli, the latter being about four hours' distant by land; the plantation villages on this side of the river extend to the lagoon behind Addah, about

two or three hours' distant, but the greater number are on the left bank of the river.

The history of the King is as follows:—

His grandfather was King Namquae, who, besides daughters, had one son, Odinko, who succeeded him.

Odinko died three years ago, and was succeeded by the present King Otabbee, who was his nephew and third son of the eldest daughter of King Namquae.

The two elder brothers of Otabbee are still alive, but their characters not being considered satisfactory by the people, neither was allowed to succeed to the stool.

According to tradition, the first ruler was a woman "who came out of the sea".

* * * * *

Rumour had reached me that King Coffee Kalkalli contemplated marching upon this town to regain possession of the vacant "stool". I did not believe in these reports, because he had been exiled, and by the customs of the Ashantis nobody who has been transported, as he was, can lawfully enter the town of Coomassie, for any purpose, until he has been permitted to re-enter by those in charge of the town for the purpose of drinking "fetische" with them, which ceremony of drinking constitutes forgiveness for past transgression.

The late King, Cobina Fuah, died about four years ago. His brother, the proper heir, being blind, was not allowed to succeed him, Cobin Fuah's sister's son, the present King, Ata Fuah (Ata means twin), was therefore placed on the stool. His twin brother is dead, but he has three other brothers living.

At one time, when Gaman and Ashanti were at war, a former Gaman King sent his royal stool and other valuable property to these people, to keep till they could be safely returned to Bontuku. By this move, though the Ashantis became masters of the country for a time, and compelled the Gaman King to pay a heavy yearly tribute of gold, they never brought the Gamans to complete subordination, since to do this it would, according to the custom of these people, have been necessary to hold the royal stool and other things emblematic of royalty, which would not only leave them the power of keeping the kingdom minus a crowned head, but would give them a much firmer influence over the masses, who regard the stool or throne upon which their former kings sat with a greater veneration than they do the heir to it.

Possessing themselves of, and retaining royal stools, has always been a favourite plan of the Ashantis. At the present moment

they claim several such stools, hoping, by obtaining possession of them, to induce the people to return to their (the Ashanti) rule.

The assemblage of the many great Chiefs to go through the formality of "shaking hands" with me, with their respective followers, was an imposing sight, and covered a large extent of ground. Some spectators, estimated at 2,000 in number, were looking on at the proceedings, and this seemed to be the population of Coomassie. There was but one thing wanted to render the gay scene more satisfactory, and that was the presence of the king on the "stool", his absence, coupled with the exhibition of the vacant royal "stool" lying on its side, instead of in its proper position, but in its right situation for such occasions, viz., on a very high raised dais, was melancholy, for it told its own tale. No conversations were exchanged.

There appears to be a conflict of opinion as to the succession to the "stool". The districts of Adukrum and Larti declare for, first the king's son and then the king's brother; the districts of Aburi and Amanakroom and Akropong say, first the king's sister's son and then the king's sister. It is probable that it is simply a matter of the election by the chiefs and people of some member of the Royal family.

The formalities connected with the custom of native marriage appear to be almost identical in the countries of Croboes and Aquapim, with the exception of making the "Otofu" custom, which is peculiar to, and almost indispensable in, Croboe.

As regards the succession to the stool, King Kofi Chintor told me that upon the decease of the king his next eldest brother succeeds him. Should he have no brother, then his nephew (by a sister) becomes king, even though the latter be a mere child. The king's son never succeeds him, as the king's wife may happen to be a woman of some other country, and consequently, if the present custom did not prevail, she, a foreigner, would obtain possession of the country in the event of her son (if he had been made king) dying, as the mother is the heir-at-law.

CORRESPONDENCE.

QUASI-TOTEMISTIC PERSONAL NAMES IN WALES.

THE ancient Welsh personal names that refer to animals (and other non-human objects) are, whatever be the explanation of them, exceedingly interesting, and it seems worth while, in view of Mr. Gomme's recent articles on "Totemism in Britain", to bring some of these names together. In doing this I have been very much helped by the list contained in Professor J. E. Lloyd's article on "The Personal Name-System in Old Welsh", in *Y Cymmrodor*, vol. ix, part i. I have, in most cases, given preferably the modern forms of the names, or what would be the modern forms of them were they still preserved. I assume that the prefix "Cyn-" of the names quoted stands for "dog".

1. BEAR. Arthgen (*Bear-born*). Arthflaidd (*Bear-wolf*).
2. CROW. Bran (*Crow*). Cynfran (*Dog-crow*). Llewfran (*Lion-crow*).
3. CORMORANT. Morfran (*Cormorant*).
4. DOG. Gwrgi (*Man-dog*). Cynfarch (*Dog-horse*). Cynfran (*Dog-crow*). Maelgwn (*Hero-dog*). Hoywgi (*Sprightly-dog*). Cyngen (*Dog-born*).
5. HORSE. Cynfarch (*Dog-horse*). Gwrfarch (*Man-horse*). Llewfarch (*Lion-horse*).
6. IRON. Haiarn-gen (*Iron-born*).
7. JACKDAW. Corfran (*Jackdaw*).
8. LION. Llewfran (*Lion-crow*). Llewfarch (*Lion-horse*).
9. SUN. Sulgen (*Sun-born*).
10. WHELP. Gwrgeneu (*Man-whelp*). Morgeneu (*Sea-whelp*). Gwyn-geneu (*White-whelp*).
11. WOLF. Arthflaidd (*Bear-wolf*).

Other names might be given, but these are well attested. A peculiarity in the case of some of them is the blending of two animal names to form a single personal name, as Cynfarch (*Dog-horse*) and Arthflaidd (*Bear-wolf*). A peculiarity of others is that they imply a non-human origin, as Arthgen (*Bear-born*), Sulgen (*Sun-born*). The lion, whose name enters into the composition of some of the personal names, is a non-British beast. These names are, I think, worthy of notice, even though the totemistic explanation of them should not be sustained.

Wrexham.

ALFRED NEOBARD PALMER.

ROOK OR RAVEN?

IN the *Archæological Review* for March, Mr. Karl Blind says ("A Fresh Scottish Ashpitel", p. 29): "In this country the raven still holds his lofty place, as of yore, under the ruler of Asgard. Witness the hosts of these birds kept near English country mansions. Their croaking voice is by no means considered an evil sound, as it is in Germany," etc. Surely Mr. Blind does not mean us to suppose that the raven of fairy story is simply a rook? I, at least, have never seen "hosts" of ravens near English mansions or elsewhere, though I have occasionally seen one solitary raven, and once the pair, about our Cornish cliffs (where, however, they are getting rare, and it will soon be as much of an event to see one as to see the chough). I cannot think the friendly and sociable rook, rarely seen solitary even for a few moments, would ever have been hit upon as "bird of evil genius" by ancient or modern.

ISABELLA BARCLAY.